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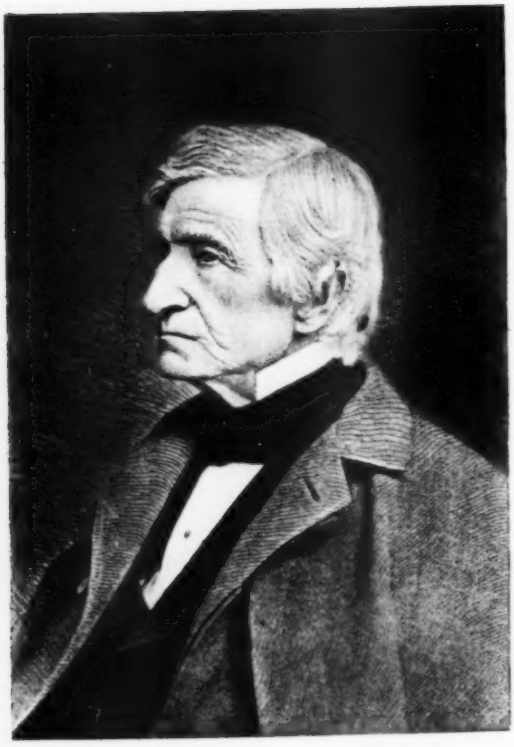

William B Rogers

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FRIDAY, JULY 2, 1897.

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MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Prof. J. McKeen Cattell, Garrison-on-Hudson, N. Y.

LIFE AND LETTERS OF WILLIAM BARTON ROGERS.

THE present generation of men and women have shown in an extraordinary degree their interest in the personality of historical characters.

Current literature teems with examples of this interest in biographical sketches without number, and the public seems to enjoy the most trifling details of the lives of persons of distinction, to secure which both the pen and the camera are driven to the last limit.

That this taste has abnormally developed and is in a large measure harmful few will deny, but there will always exist a wholesome curiosity as to the personal qualities of men whose performances have commanded our admiration. It is not alone in political history that biography is so important. To the student of science a knowledge of the temper, spirit, moods or idiosyncrasies of eminent scientific men has always been highly valued, not only on account of its intrinsic interest, but also because such knowledge often assists in a proper interpretation and appraisement of their work. But when distinguished success is to be attributed to personal qualities even more than to talent; when to scientific accomplishments of a high order is joined a personality so charming and so noble as to silence opposition and command approval, anything that reveals, even a small

degree, the motives, inspirations or promptings of such human activity is doubly welcome.

The 'Life and Letters' of Professor W. B. Rogers, edited by his wife with the assistance of Professor W. T. Sedgwick, has recently made its appearance.* The memoir is published in two beautifully printed volumes of about 400 pages each. In the short preface to the first the editors explain that they have been prepared in the hope that they 'may be of some service to the cause of science and education.' Their work has been done with rare wisdom and good taste. It has consisted mainly in providing a continuous yet unobtrusive background of biography, upon which is displayed an extensive and wonderfully interesting series of letters, in which the whole story of the life of Professor Rogers is revealed.

It may be well in the beginning to briefly summarize in their chronological order the principal events of that life. Professor Rogers was born in Philadelphia in 1804. His father was Dr. Patrick Kerr Rogers, who was himself born in Ireland in 1776, the son of Robert Rogers, of Edergole. Mr. P. K. Rogers fled from Ireland on account of articles which he had contributed to the Dublin newspapers in criticism of the action of the government in the matter of the Irish Rebellion of 1798. Coming to Philadelphia he entered the University of Pennsylvania as a tutor, and in 1802 he received the degree of Doctor of Medicine from that institution. A year before graduation from the medical school he married Hannah Blythe, who had come from Glasgow to make her home in America. She died in 1820, leaving four sons, James B., William B., Henry D. and Robert E. Rogers, all of whom became eminent in science and in public life, being frequently spoken of in Europe and in this country as 'the brothers Rogers.'

Dr. P. K. Rogers removed with his family to Baltimore in 1812, and in 1819 he was elected to the chair of natural philosophy, which at that time covered a very broad field, in William and Mary College, Virginia. William was a student here in 1819–20, and there is printed in the memoir an interesting fac simile of a report of the standing of the two sons, James and William, in which, in addition to the statement that they had been 'orderly, diligent and attentive to their studies,' there is an interesting forecast of their career in the declaration that 'in mathematics and chemistry they have made the most flattering improvement.' There is good evidence, some of which is found in the fragments, which have been preserved, of a correspondence with Thomas Jefferson, that Dr. Patrick Rogers was an accomplished mathematician and general scholar and that the tastes and talents of the 'brothers Rogers' were largely inherited. When James, at the early age of 19 years, was studying medicine in Baltimore he wrote to William, aged 17 years, a letter full of criticism of the 'singular views' advanced by one of his professors on the subject of chemical attraction or affinity and the true cause of chemical union. He begs William to give him his own opinion of the matter in an early reply, and concludes with a burst of filial loyalty in the declaration that, although the aforesaid professor is the best lecturer in the institution, the lectures of their father on the same subject as far surpass his work 'in point of correctness, science and elegance as the meridian sun does the evening star in brilliancy.' In a youthful oration given by William on a public occasion in 1822 he foreshadowed, at the age of 18, the eloquence of speech for which he was in later years famous.

* Published by Houghton, Mifflin & Co., Boston, to whom we are indebted for the plate of the portrait accompanying this article.

In 1825 both William and Henry removed to Baltimore to seek their fortunes, but the physical weakness by which they were greatly handicapped and against which they fought during all of their lives interferred with their success from the start. The two brothers finally obtained employment as teachers, but there was frequent necessity for pecuniary aid from the father, who was ever ready to draw upon his slender salary the payments of which were made with much irregularity and often much delay, for the benefit of his sons, of whom he was extremely fond and justly proud.

William began to show his rare qualities as a lecturer on Science, and in 1827 he was appointed to a lectureship in the Maryland Institute. His father died in 1828 and in the same year he was elected as his successor in the chair of natural philosophy at William and Mary, at the early age of 24 years. Here he remained for seven years, growing in knowledge and reputation, and in 1835 he was elected professor of natural philosophy in the University of Virginia, and shortly afterward he was appointed State Geologist. His connection with this institution continued for eighteen years, and its history forms one of the most interesting features of the memoir. During this period the University passed through some of its most notable trials in connection with the relations of its governing authority, the faculty, to the student body. In all of these events, as well as others of more lasting importance to the institution, Professor Rogers was a prominent figure. That he impressed himself in a notable way upon the then young and developing University was always recognized by its officers and friends. That his connection with it, and especially his long life in the South, covering as it did his youth and the most impressionable years of his manhood, did much to influence his educational ideas as well as his personality, is equally true. Indeed, there was always in his manner something of that courtliness and chivalry which we are wont to associate with the cultivated Southern gentleman and when transplanted to New England it became one of his principal charms. During his life in Virginia his fame as a lecturer and scientific investigator grew continually. He was invited to lecture in various cities, and just previous to his leaving the South Professor Henry solicited his assistance in a course to be given 'for the benefit of Congress' at the Smithsonian Institution.

In 1849 he married Miss Emma Savage, of Boston, going with her to Europe, where some pleasant months were spent and valuable acquaintances formed or renewed, and returning to the University of Virginia in October of the same year. In 1853 he resigned his professorship and removed to Boston, where his wife had lived and where her family and friends were, and also where his brother Henry had already met with such generous support and assistance in his scientific work. Here, for a period of nearly ten years, he worked, wrote and lectured, all the time keeping in mind the organization and development of a school of technology or applied science, the plans for which had been in his mind since at least as early as 1846. On April 10, 1861, the Act incorporating the Massachusetts Institute of Technology received the approval of Governor Andrew, just as the Nation was plunging into what proved to be a mighty struggle for its existence. A year later he was formally elected President of the Institute, which as yet had no material existence. Indeed the war for the preservation of the Union delayed the consummation of his desires until October, 1865, at which time instruction in the new school was actually begun.

In 1868 Professor Rogers began to show the effects of the years of devoted activity which had resulted so fortunately as far as

the school was concerned, his always rather delicate health giving way to such an extent as to make a prolonged period of rest imperative. Accordingly he resigned his presidency of the Institute in 1870, and lightened his labors as much as was possible. In 1876 he presided over the meeting of the American Association for the Advancement of Science in Buffalo, a meeting which was distinguished by the attendance of many foreign savants who were drawn to this country by the Centennial Exposition, held that year in Philadelphia. He had been specially chosen by his fellows to preside on that occasion, because of their desire to put forward the best available representation of American science.

In 1878, on the resignation of Professor Runkle, who had succeeded him as President of the Institute of Technology, he was induced to accept that office again, with the understanding that his holding should be but temporary. In 1879 he was elected by the National Academy of Sciences to succeed Professor Henry as its President, the highest honor which can be conferred upon a man of science in this country. In 1881 he again resigned his presidency of the Institute of Technology, General Francis A. Walker having been selected by him and appointed on his recommendation as his successor. A year later, at noon of May 30, 1882, came his tragic death. In the midst of an address to the graduating class of the Institute, in which his hearers were delighted with an apparent revival of the spirit and eloquence with which he was accustomed to enrich every occasion for dignified address, he fell upon the platform of Huntington Hall, surrounded by the material realization of his dreams of nearly forty years earlier and by those who by the closest associations had learned to love him as few are loved in these days.

Fit into this skeleton sketch the interesting letters which happily have been preserved, mostly being correspondence with his brothers or family friends, and the story of his long and useful life will be fairly well told, although to form an adequate idea of his accomplishments in science and education one must consult the appendices to the two volumes where will be found a list of his scientific publications and one or two of his more important educational papers. I may venture, however, to give the reader of this notice a little taste of what the volumes have in store for him, and at the same time illustrate the remarkable biographical value of the published letters by quoting from a few of them.

Mention has been made of his power as a lecturer on scientific subjects and of his ability to stir with eloquence even when comparatively dry and interesting material was under discussion. One of his early pupils at the University of Virginia says of him that he was endowed with "not alone an imagination which ever clothed truth with beauty and made the dullest fact radiant with a significance illimitable and imperishable, but also every personal gift which can enhance the power of the orator. Tall in stature, with a figure of the type known to us through the pictures of Henry Clay; with a face that, destitute of all assumption or arrogance, was singularly commanding; with a voice whose compass and quality were capable of producing at once the largest and the finest effects of speech, William Barton Rogers was, in the height of his powers, without a peer among the scientific men of his age in addressing an intelligent and cultivated audience." This remarkable *faculty for teaching* was appreciated by all who came in contact with him. Theodore Parker wrote to him in 1859, when just on the point of starting for London: * * * * * * * "I return Mr. Owen's remarkable pamphlet. What an instructive thing it is!

"I should have been surprised that it could all have come from one man if I had not known yourself and your brother, who in such matters taught me the *nil admirari*. * * * Allow me to thank you for the instruction I have received from you, and for the many friendly and noble words you have spoken to me. My acquaintance with you began with your brother, and I feel gratitude to you both. For you both turn your deep, wide science into humanity. I have found you both always on the side of mankind and feel strengthened and encouraged by your example." Wherever and whenever good speaking was understood and appreciated Professor Rogers was in demand. When in England, in 1849, he attended the annual meeting of the British Association for the Advancement of Science, and in the presentation of a scientific paper his extraordinary power of exposition was at once recognized. He was put down for a speech at the great dinner which celebrated the close of the session, and in a letter written on the morning of the day on which it occurred he wrote: "I suppose I shall be compelled to show my Yankee 'gift of the gab.'" He met with a flattering reception at the hands of such men as Darwin, Faraday, Murchison, Sedgwick, Brewster, Mallet and Adams. He writes to his brother: "You may imagine how my heart beat to hear your name so honored and to have our labors so warmly eulogized."

The younger Mallett, who was present on the occasion, afterwards said of it: "Although I was but a boy at the time, attending the meeting with my father, I recollect most distinctly the marked impression made on the large assembly by Professor Rogers's speech and the enthusiasm it kindled. It came late in the evening, after much, perhaps most, of the matters appropriate to the occasion had been utilized by others, yet it was clearly *the* success of the banquet. Americans were less known in England than they have since become, and the slight foreign flavor which accompanied a speech, excellent in itself, and fluently delivered in the mother tongue, added to the piquancy and effect." The orator, like the poet, is hardly a manufactured product, but all who have occasion to lecture on scientific subjects will be greatly interested in and benefited by the many hints as to his own ideas of how difficult subjects may be clearly set forth, which will be found in Professor Rogers's earlier letters to his brothers.

In a letter to his brothers, James and Robert, written from Virginia in 1841, he writes as follows: "Do not attempt to crowd too much into a single lecture, and avoid the common error of experimenting for the eye and not for the understanding. Every experiment ought to be accompanied by a full and clear explanation, and this cannot be too explicit and elementary. Cultivate a deliberate and distinct enunciation without sacrificing earnestness and animation of manner. Above all, do not attempt to be over choice in your phraseology, but use the language suggested at the moment. * * * * In my view, the very first thing to be sought is a feeling of ease and confidence, and this, when the subject is thoroughly understood, you cannot fail to secure at the outset by giving play to some enthusiasm, and, as Rutledge advised, 'speaking right on,' even though at times your phrase may be obscure, inelegant or even incorrect."

The whole of his extensive correspondence with his brothers is filled with evidences of the intense affection and brotherly love which existed among them. They understood and appreciated one another. In 1822 James, aged 20 years, wrote to William, two years his junior: "I have, I think, perceived in Henry (aged 14 years) that constitution of mind which is admirably fitted for success in this world and which, if properly cultivated, would mani-

fest genius of no ordinary cast." Some years later William wrote: "* * * But by and by, my dear Henry, we shall, I trust, be able, shoulder to shoulder, to win a position in which we may enjoy ourselves in science and society, free from all anxiety and in a spirit of entire independence." Again on a Christmas day several years later: "My heart longs more than I can express for the coming time when we may all spend together, as in childhood, these festival days, and when we shall always be so near as not to feel the sense of separation."

Both Henry and William struggled against many difficulties, the former in Pennsylvania and the latter in Virginia, to secure the legislation necessary to the execution of a geological survey, and their experiences, as recounted in their correspondence, may serve to encourage those who are struggling towards similar results in more modern days. In 1841 Henry sent to William the complete verbatim report of the speech of one Senator at Harrisburg, one who in private conference had promised his support, which was as follows: "Mr. Speaker, I shall vote against this appropriation, on the ground of its unfairness to other sciences of like nature to this geology. The bill, sir, makes no provision for phrenology, physiognomy, animal magnetism and the highly important science of *water-smelling;* it is partial and I shall vote against it." Both William and Henry had rare gifts of argumentative power and it was exceedingly uncommon for either to fail in carrying with them men whose support was needed. It was his eloquent earnestness and unselfish enthusiasm that brought to Professor Rogers the greatest success of his life, the establishment of the Massachusetts Institute of Technology.

The first plan for the organization of this institution he drew up early in the year 1846, several years before he left the University of Virginia. In a letter to his brother Henry he says: "Ever since I have known something of the knowledge-seeking spirit and the intellectual capabilities of the community in and around Boston, I have felt persuaded that, of all places in the world, it was the one most certain to derive the highest benefits from a polytechnic institution." When in 1860 he thought the time had come for an active prosecution of his scheme he was tireless in his labors, preparing and submitting plans and reports for the Legislature, delivering public addresses on the importance of technical and scientific training, enlisting support from men of wealth and influence, becoming, in fact, himself the embodiment of the whole enterprise. It is unnecessary to say that the story of the last twenty years of his life is, in a large measure, the history of the first twenty years of the Institute.

The frequent references in his correspondence to the intellectual and scientific activity of the time are extremely interesting. To one accustomed to the quieter and more deliberative methods of the South in all matters of a literary or scholarly sort the intensity of life in Boston was something of a revelation. Henry Rogers was the first to taste of this and he wrote to his brother William as follows:

"For a man of any brains whatever, Boston has no peace or quiet; all is restless excitement and unproductive change of thought and of pursuit. The overworking of the brain here without the fruits of intellectual labor is appalling to a mind of contemplative tendencies. Often do I envy you and Robert your calmer studious atmosphere."

The active dissipation of mental energy thus portrayed may not be entirely absent from the intellectual life of the 'Huf' even to-day, but Professor Rogers was not frightened by its existence forty years ago and he speedily adapted himself

to his changed environment. Although he shortly became a prominent figure in social as well as scholarly circles he still retained his fondness for real, hard scientific work, and during his busiest life in Boston made many scientific investigations of great interest and importance.

His personal acquaintance with eminent men of science on both sides of the Atlantic was extensive and his occasional references to them in his letters add greatly to their value. It was in the earlier years of his connection with the University of Virginia that Professor Sylvester came from England to succeed Bonnycastle as professor of mathematics. Professor Rogers had for some time after the death of the latter carried on the work of the department and he was naturally much interested in the arrival of his relief. He wrote: "For several days we have been anxiously looking for the arrival of Professor Sylvester; we learn he lost all his baggage in Boston; this may have detained him"—which goes to show that some of the personal peculiarities of the distinguished mathematician were not confined in their exhibition to later periods of his life. Again, a few weeks later: "The faculty, students and others attached to the University are all greatly pleased with Mr. Sylvester. He was terribly embarrassed at his first lecture, indeed quite overwhelmed, but has been doing better since. He has a good deal of hesitation, is not fluent, but is very enthusiastic and commands the attention and interest of his class." His brother, Robert E. Rogers, who had for some time filled the chair of chemistry in the University of Virginia, was appointed in 1852 to the professorship of chemistry in the University of Pennsylvania, made vacant by the death of his brother James. He was succeeded in Virginia by Professor J. Lawrence Smith, of Kentucky, already a chemist of distinction. He brought with him two young men as assistants, George J. Brush and Ogden N. Rood, both of whom were to achieve distinction in later years. Of the first Professor Rogers wrote in 1852: "Young Brush is a zealous mineralogist of the Yale School, and seems to be familiar with all parts of chemical analysis. He talks a great deal and very admirably of young Silliman and Dana, and I find that he supposes New Haven to be the great center of American science." In 1853, after having resigned his professorship in the University of Virginia, he wrote concerning the well-known professor of physics in that institution: "My successor is young Mr. Smith, the mathematical tutor, and a favorite pupil of mine."

In 1857 Henry D. Rogers was appointed to a professorship in the University of Glasgow, Scotland, and the correspondence of the brothers became more interesting on account of the wider range of personal acquaintance and association which naturally resulted. In 1859 William wrote to Henry: "I read William Thomson's speech with great interest, and should be glad to see him operate with his ingenious instruments. It is rare to see such profound mathematical knowledge united to such skill in its application to physical questions, and such ingenuity in mechanical contrivance." It would be difficult to find a better description of Lord Kelvin than this. Henry had lectured at the Royal Institution, and on March 11, 1856, William wrote: "Tell me in your next what sort of an audience you had at the Royal Institution. What is Faraday about just now? and Wheatstone? Tyndall seems to be taking a leading part at the Royal Institution. He has fine talents and I hope he is a good fellow; but where is there another Faraday?"

An interesting letter dated August 1, 1860, from James Russell Lowell, who was then editor of the *Atlantic Monthly*, reveals the fact that Professor Rogers had sent him

a list of remarkable blunders in an article on meteorology which that magazine had published. The criticism is accepted in a most good-natured fashion, and Mr. Lowell explains how he proposes to make acknowledgment of the mistakes in the *Monthly*. It is to be done by a humorous editorial, in which the blame for accepting the unfortunate article is to be thrown upon the 'Æsthetic Editor,' who ran the establishment all alone, during the absence of the 'man of science;' and all this, Mr. Lowell says, because "no right-minded magazine can allow itself to be corrected *ab extra*." One might almost conclude that modern journalism had learned some of its lessons from this venerable and always proper and respectable periodical.

A note from Charles Sumner, written at Washington in 1863, reveals something of the anxiety by which men's minds were filled in those days. It concludes: "I wish I could talk with your brother for half a day. Remember me to him most kindly. I know he keeps his loyalty. But what does he say of England—our England—and her shortcomings?"

Indeed there is nothing in these volumes more impressive or more worthy of the distinguished man whose life they unfold than the continued utterances of patriotism and loyalty in which his letters during the war period abound. In giving full weight to this statement it must be remembered that the greater part of Professor Rogers's life was spent in the South; that he grew to maturity amid Southern influences and that he numbered among his personal friends many who were afterwards prominent in the attempt to overthrow the government.

During the early 'fifties' there are frequent references in his letters to the storm which he distinctly saw was threatening the Republic. His faith in the indestructibility of the Union was lasting and strong. In 1856, writing of the difficulties in Kansas, he said: "I am, however, of those who think that our Union is too strongly framed in Constitutional right, and bolted together by mutual interest, to be severed by even such a shock as this." In 1858 he wrote to Henry: "I see with sorrow and indignation that Senator Mason contemplates some general provision for bringing new States into the Union by *pairs*, so as to maintain the present balance between the slave and free States! But this *cannot be done*." Early in 1861, again to Henry: "The fears of State Street and the prejudices of Beacon Street may have some effect, but the great mass of New England and, I think, of the free States in general, will refuse a compromise which claims national protection to slavery far beyond the extent of the present Constitution." And a few days later: "Should the Gulf States remain out of the Union I see little reason to expect a better fate for them than is seen in the degradation, destruction and fickleness of the South American Republics."

During the years in which the Civil War was waged Professor Rogers was especially active, through his brother Henry, in striving to enlighten the people of Great Britain as to the real situation in this country, and there can be no doubt that the excellent social relations which the two brothers enjoyed with people of rank and influence in London, Edinburgh and Glasgow contributed largely to this end. Immediately after the firing upon Fort Sumter William wrote to Henry at great length, saying, among other things: "It is of great importance that the position of the free States and National government in this contest should be truely known in England. Every word of sympathy, or even of toleration, for the South arising from Europe, and especially from England, is magnified by the excited people there and does great harm. * * * It grieves and mortifies me to see several of my old

friends and pupils among the most passionate leaders in this revolt."

Professor Rogers found many channels in which to suppress his feelings of loyalty and patriotism, and the newspapers of the time record the fact that only a week or two after the outbreak of hostilities when he was called upon to speak at the Thursday Club on some matter pertaining to science he "very gracefully declined to discuss the topic proposed, and then made a stirring appeal to the Club in favor of providing a regiment of our brave volunteers with knapsacks." This appeal was seconded by the Hon. Edward Everett, the President of the Club, and in a few minutes a thousand dollars were subscribed.

Innumerable examples showing this intense spirit might be quoted, not alone from letters to his brother in Glasgow, but from those to many prominent Englishmen whose friendship he enjoyed. During the darker days, in spite of his feeble health, he made patriotic addresses on Boston Common and on September 26, 1862, he referred to the Emancipation Proclamation of President Lincoln in the following thrilling words, a part of a letter to his brother Henry: "The great event since my last letter, the greatest event beyond comparison of the war, is the late proclamation of the President, declaring the slaves of all rebellious States after January next to be *forever free*. On the 22d of September this momentous voice was uttered. On that day—in a sublimer sense than ever before—*the sun crossed the line*."

Under date of July 5, 1863, he describes the celebration of the 'Fourth' in Boston. Dr. Holmes gave the oration in the 'great theatre to an audience packed to the dome,' and the enthusiasm was great, all hearts being absorbed in one feeling of patriotism.

As evidence, however, that his interest in the progress of science was by no means dormant during those potentous days, he adds: "What kept me in the city, however, was my interest in the exhibition of the electric light, which the Council, at the instance of George Hale, encouraged by me, decided to make one feature of the evening celebration, as a substitute for part of the usual fireworks." He describes in interesting detail how Ritchie accomplished a superb success by putting a battery of 250 cells on the top of the State House dome, from which a 'flood of light' was thrown upon over 100,000 pedestrians who thronged the streets.

But surely enough has been quoted and enough said to give some notion of the extreme interest of these volumes, not only to scientific men, but to all intelligent people who admire exalted character and lofty sentiment. America has produced no finer type of man than was exhibited in William Barton Rogers, who showed that it was not impossible for one who was primarily devoted to learning and original research in pure science to be at the same time a lovable companion, an eloquent lecturer and a man of affairs whose influence upon his contemporaries resulted in the creation of new institutions and the remodeling of old. It is even now too early to recognize in full measure the value of his life to the people, to whose best interests he showed a rare fidelity.

T. C. MENDENHALL.

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*THE MERTON RULES.**

THESE are the Code of Nomenclature "at present in force for regulating all work done in the study of Microlepidoptera at Merton," and "the object of these rules is to insure absolute obedience to the Law of Priority." In so far as this Code is pecu-

* Rules for regulating Nomenclature with a view to secure a strict application of the Law of Priority in Entomological work. Compiled by Lord Walsingham an [and] John Hartley Durrant. Longmans, Green & Co.: London, New York and Bombay. 2 Nov., 1896. 8vo, pp. 18.

liarly or exclusively adapted to the exigencies of entomological work it is of course not within the purview of ornithology; but its most avowed object of insisting upon the Law of Priority, its whole tenor, and most of its fifty-one canons, are no more pertinent to one branch of zoology than to any other. Lord Walsingham and his secretary thus submit a set of rules to the consideration of all zoologists, and no apology is needed for examining them with special reference to the A. O. U. Code.

Naturalists are in substantial agreement upon such a large majority of the propositions which bear upon the 'highly-contentious' subject of Nomenclature that we should expect any code drawn up by competent workers to be good in the main. Such is emphatically the case in the present instance. If, therefore, we seem to dwell upon the exceptions we take, to an extent disproportionate with the commendation we give, it will be understood that the latter goes without saying. Probably four-fifths of these rules will receive general unqualified assent; it is mainly regarding the remainder that we have here to do. Most of these may be reasonably questioned, and some of them are likely to be regarded as highly objectionable.

We think that the authors make a strong point in the introduction, but it is made so quietly that its full force may not be recognized at first sight. This is where (p. 4), referring to other codes, the authors say they "are not aware that in any of these rules the actual work and intention of an author has been guarded to the same extent as the names which he has given to his conceptions," and then add: "It has been one of our objects to define a method by which the recognition of antecedent work can be consistently secured, regarding this also as no less a moral obligation capable of being met by the same rules which apply to mere names."

This raises a large question, concerning which we are heartily on the side of the authors. We have too often heard it said that, as we cannot enter into the inner consciousness of another, especially if he have lived in other times and been long dead, we have no concern with his spirit, purpose or intents, but only with his acts, and that consequently we must go by what he actually did, without regard to what we may think he intended to do. We believe this to be bad—unjust and dangerous; Lord Walsingham has the right of it in introducing something of a moral or ethical principle, difficult as such may be of application in all cases. An author's intent or purpose can generally be fairly presumed or inferred from his writings; when such presumption or inference is reasonable it surely should not be disregarded, and the cases must be very few in which no meaning is discoverable.

Example: In founding his genus *Dendragapus* Mr. Elliot intended to separate grouse of the *obscurus* group from those of the *canadensis* type. That was the sole intent, purpose, scope and function of his generic name. What right, then, has any subsequent author to use *Dendragapus* for the sole purpose of uniting *obscurus* with *canadensis*? None; such a travesty of the generic name, such a perversion, or rather reversal, of Mr. Elliot's express purpose, is simply nomenclatural hocus-pocus, and as such it is puerile, unscientific and immoral.

There is another point in Lord Walsingham's introduction we must pause to applaud heartily. Though it be one of those which, as we say above, go without saying, it is particularly well said (p. 5): "The object of all rules should surely be to secure precision, uniformity, and finality: any sacrifice of these objects to considerations of mere convenience can only result in the creation of greater inconvenience at some future date. The inconveniences from which

workers in entomology now suffer would have been entirely avoided had the earlier authors studied and recognized the work of their predecessors, or adopted some such rules as are here set forth, beginning from the time of Linnæus. The earliest sinner in this respect was Fabricius, but Stephens in creating the necessity for Rule 48, has exhibited even greater ingenuity in his aberrations." Here, again, is an obvious ethical principle. We do owe something to posterity, notwithstanding the question which some wit once asked: "What has posterity done for us?" This debt is in the nature of a moral obligation not to consult our own present convenience at the expense of those who are to come after us. And if we may disregard the altruism involved, then self-respect and an enlightened selfishness should alike prompt us to the same end, merely as a matter of looking out for our own good repute.

A part of Lord Walsingham's 'Premiss' will be hailed with acclamation by almost every American zoologist. It is as follows:

"Zoology became an intelligent science when it was recognized that every species should possess a special name and every genus a generic name. This system of nomenclature was first enunciated by Linnæus in the 'Systema Naturæ' (edition X.), 1758; and as we owe the conception of the special and generic name to Linnæus, we are bound to commence our nomenclature from the year 1758, when he published his epoch-marking work."

Shade of Strickland! Requiescat in pace! We do not propose to echo an anthem to this requiem, nor even to argue the point; for we could say nothing that has not been said fully and perfectly well in the A. O. U. Code itself in support of this reasonable proposition—one of self-evident logical necessity; and if what is recorded there does not budge our British friends, nothing will move them from the isolation of their insularity. We know that most of our respected colleagues on the other side of the herring pond still stand on the rock of offense, whence to denounce with objurgation those who do not subscribe to the B. A. Code. Possibly we undertook to split that rock with the butt end of our wedge; probably Lord Walsingham may prove to have insinuated the thin edge from his own coign of vantage. He may not be the first among English naturalists to favor the heresy of 'Linnæus at '58;' but he is certainly one of the strongest, and much may be confidently anticipated from the force of his example. We remember once discussing with him in person the 'American idea' of Trinomials. We may have been persuasive, though we failed to be conclusive, in our presentation of that case to his liberal and progressive judgment; but the stand he has now taken against the extreme conservatism of his countrymen leads us to the confident hope of his enlightenment even upon those 'dark sayings' of Trinomialism. If the scales could fall from the eyes of such a one as Saul of Tarsus, a Paul of London, Cambridge or Merton may not be a zoological impossibility in the course of natural evolution.

Passing by most of the Merton rules as self-evident, or as admitted by the concensus of naturalists, or else as peculiar to microlepidopterists*, we note some few

* A valued entomological friend of high standing, who has not authorized the use of his name in this connection, writes to us regarding some of these: "I think that most entomologists would take exception to the group of rules beginning with No. 19 and ending with No. 25, except that No. 23, since it begins with the words 'it is advisable,' is acceptable. Similarly, under No. 33, *e. g.*, Zeller's correction is not admissible. Under No. 38, the third paragraph, indicating that the type is the sum of the co-types. Mr. Oldfield Thomas's terms under No. 39 have not come into general use, though they are sensible enough. Under No. 41: No one accepts the idea that the type of the genus is the sum of the species as under 2 and 3."

others for special approval or the reverse.

Rule 18. "If it be held that the generic and special names may not be tautonymic, the law of priority will determine whether the special or the generic name should be changed, *e. g.*, *Cossus cossus*, L." The proviso in this case nullifies the rule for us, because we do not forbid tautonymity. Our practice is bad—obviously so, on the score of literary propriety; it is to be strongly discouraged; but it seems an unabatable nuisance, which most naturalists will put up with perforce, and it has one redeeming feature—we know absolutely what every tautonym means.

Rule 19, bringing up 'three classes' of invalid names, is likely to remain a bone of contention in one-third of its scope. We all agree regarding homonymous names, that they are absolutely to be rejected; so, of course, regarding synonymous names. But not so regarding homophonous words, for surely no one of us would reject *Seiurus* because its sound when spoken is identical with that of *Sciurus*.

Rules 20 and 21, hanging on the foregoing, are open to difference of good sound opinion, possibly because, for one reason, it may not be always clear what words come under these provisions. It is desirable, but probably impossible, to have rigid rules here. No rule can possibly be more rigid or stable than the sum of the cases to which it applies; but the cases intended to be covered by Rules 20 and 21 are so shifty and mobile that no seive can be devised with meshes fine enough to catch them all. We confess ourselves puzzled here; we cannot offer better rules than Lord Walsingham has, yet we doubt their sufficiency. It seems to be a case where common sense, tact and expertness may work better than any formality. Let us agree, as most, if not all, ornithologists would, that *Telea* invalidates *Teleia*, and *Pandemos* invalidates *Pandemis*; does *Ucetia* invalidate *Eusesia*? Personally, we should say that would depend upon the etymology in the case. If *Ucetia* be merely a bad way of spelling *Eusesia*, the etymon in the two cases being identical, we should say they were the same word, not available for two names, for all that they happen to be spelled so differently. We would, therefore, spell to the best of our ability both names as one, and use the right spelling or not according to the provisions of some other rule regarding homonyms. But if *Ucetia* and *Eusesia* be of different etymons, only accidentally homophonous, we should regard them as distinct words, neither of which would invalidate the other. Yet we know that others might take exactly the opposite view and argue strongly in its favor. As we said in substance, this whole class of cases has thus far proven refractory to, or elusive of, any rule naturalists have had the wit or ingenuity to devise.

Rule 22. "A name wrongly written is invalid if, on legitimate correction, it becomes homonymous or homophonous with a valid name; *e. g.*, *Grapholitha*, Hb., invalidates *Grapholita*, Tr." The A. O. U. Code would have simply to quash this rule, because it forbids all correction of names, 'legitimate' or illegitimate. We think it is the very worst blot on our Code, which has done more to bring that work into disrepute than all our other weak or bad points put together. It puts a premium on ignorance or carelessness to rule, as we do in Canon XL., that a name shall endure exactly as it originally appeared in print, no matter how mangled it was, unless a *typographical* error be evident. Why be so fierce with the poor compositor, and let the guilty author go scot free? We need not become formal impurists for fear of purism. Personally, we decline to bind ourselves to misspelt words forever, for no better reason than that some zoologists, too ignorant or

too slovenly to spell them properly, sinned in the beginning. We are opposed to 'original sin,' whether as a theological dogma or a canon of nomenclature. On this subject we cite from a private letter lately addressed to us by one of the most learned and distinguished of American philologists, Professor C. P. G. Scott:

"I think you are quite right, as a scholar, in your disapproval of the mechanical rule which, as a member of a committee constrained to compromise, you passively sanctioned in 1886. It seems to me that these verbal uncertainties will *never* end, no matter what committees may recommend or do. Therefore, since the only purpose of compromise is to end uncertainties and discrepancies, it would be better for your committee to revise the code of nomenclature with reference to etymologic principles which *can* be ascertained and stated, and to consider all material deviations, intended or unintended, old or new, as if misprints to be corrected in subsequent works. Etymologic principles are a good deal more stable and visible than is commonly supposed; but you know by long observation that not every zoologist (particularly in France) is sound in his tackling of Greek and Latin."

Our zeal for 'spelling reform' might not now lead us to the length it did, for example, when we emended Richardson's genus *Aplodontia* into *Haplodon*, only to find, to our dismay, that in its new guise the name was homonymous with several others of prior date, and therefore inadmissible for the genus of mammals. We would treat such a case as incorrigible, and let it go at that, without regard to its etymology. It seems to us that tact, discretion and common sense, applied to each individual case, is likely to work better than any rigid rule which could be devised to cover all cases. We are not such rigid purists as to sacrifice the Law of Priority to purism. In fact, we would not go to the length Lord Walsingham does in '33 *e. g.* (2),' where he 'corrects' *cretidactylus* into *gypsodactylus*. If both these names mean 'chalk-toe,' as we suppose, the substitution of gypsum for chalk, to prevent a Græco-Latin hybrid, seems hardly required. That is mere purism, only less unobjectionable than the systematic impurism which the A. O. U. Code would force upon us. A certain 'sweet reasonableness' would seem to be the best prophylactic or preventive—better than extreme measures either way. Such heroic treatment is likely to become the mock heroism of opera bouffe. As Horace said, some years ago:

> "Est modus in rebus; sunt certi denique fines,
> Ultra citraque nequit consistere rectum;"

and as Professor B. G. Wilder lately remarked, in the course of his controversy with Wilhelm His:

"As with biologic generalizations, there are few philological rules without exceptions. Yet the reformer, especially if young and enthusiastic, either ignorant of history or undismayed thereby, 'too often imagines that a principle, if right, cannot be carried too far.'" (Barclay, 1803.)

Our A. O. U. committee may be neither very young nor over-enthusiastic; yet this is precisely what we have done to the excellent principle of priority—carried it too far, in attempting to impose verbal abortions upon nomenclature. The results sometimes better befit the nursery in their puerility than the halls of science. Take that miserable botch of a word *Leptotila* Swainson for a genus of pigeons. If the celebrated quinarian had printed *Leptoptila*, in proper form, his genus would have been invalidated by the prior *Leptoptilos* or *Leptoptilus* for a genus of storks, because by our rules a difference of termination in words etymologically identical does not prevent homonymity; but because Swainson or his printer did not mind his *p*'s—whatever he

may have done with his *q*'s—*Leptotila* [becomes valid and tenable!* This is mere juggling with the letters of the alphabet; it is absurd, undignified, infantile. We should say, first spell *Leptoptila* correctly, according to its obvious formation, and then decide on other grounds whether or not it be in this form different enough from *Leptoptilus* or *Leptoptilos*. Literary abortion should not be viable in the language of science.

Rule 23 allows difference in termination of words to make formal difference in names, *e. g.*, *Sciaphilus* in Coleoptera and *Sciaphila* in Lepidoptera, on the ground that no confusion results. How it would be did these two fall in the same order of insects is not stated. We believe the practice of entomologists has generally been to allow even identical names to coexist in different orders, on the same ground. Probably the latter state of the case is inadmissible in any other branch of zoology; but on the question whether difference of termination or inflection, as indicating grammatical gender, shall suffice to distinguish names, much might be said either way. Our practice is against it. Yet there stand our *Picus* and *Pica*, and we may yet have to reconsider our present canon on this question. Certainly *Leptoptila* and *Leptoptilus* are better distinguished from each other than *Leptotila* would be from *Leptoptila*. One is a distinction with a real difference, viz., of gender; the other is a distinction with a bogus, spurious, bastard, abortive, illegitimate apology for a difference.

Rule 24. "A name which involves a false proposition is invalid and may be changed." One would think this a self-evident proposition in science—a truism, to adult minds, hardly requiring statutory provision. The reverse would be, or should be, unthinkable in science. Yet so far afield in following the *ignis fatuus* of verbal veneration for the fetish of 'priority' have some of our codifiers been led that they would not dare to correct an error of scientific fact for fear of disturbing the cerements of verbiage in which it was embalmed! Let some one describe an albino crow as *Corvus albus*; shall we go on calling black white to the end of the chapter? Let some one describe a broken-billed popinjay as *Picus semirostris*; shall *semirostris* be the name of any species which has a whole beak? Let Gmelin describe a Mexican woodpecker as *Picus cafer*; shall we declare every time we write the name that it is a South African bird? Yet the last is exactly what we do in the A. O. U. Check List, where *Colaptes cafer* stands in a catalogue of North American birds. It is *brutum fulmen* for us to declare that a name has no necessary meaning. Such declarations simply beat the air in a futile and fatuous manner. '*Cafer*' has no meaning for those who do not know what it means, but those who do know what it means can no more divest it of its reference to a South African locality than they can take away from *Colaptes* its implication of carpentering. Granted that we have plenty of nonsense-words in science—mere combinations of letters, *sensu carentes*; granted that we have to put up with them, and that they do very well, like Smith or Jones; that does not make *cafer* mean Mexican, or *albus* mean black. The proposition is false, in form and in fact; and falsity is foreign to science. Any attempt to save the Law of Priority at such hazard, or im-

* We are sorry to observe that the A. O. U. is not consistent with itse f, even in wrong-doing. Thus: (1) *Leptotila* is held to be tenable, from lack of the *p*. Had it been *Leptoptila* it would have been untenable, from similarity to *Leptoptilos*. (2) *Fregetta* Bp. is held to be untenable, owing to a prior *Fregata* of Brisson, and *Cymodroma* is used instead. (3) But *Fregetta* is quite as different from *Fregata* as either *Leptotila* or *Leptoptila* is from either *Leptoptilus* or *Leptoptilos*; *i. e.*, there is no real difference in either case. The A. O. U. is wrong in one of these cases, necessarily—which one?

pose uniformity of nomenclature by the arbitrary authority of an individual or a committee who should go to such a length as to say either that words have no meaning or that we shall use words in wrong senses must fail, for the simple reason that common sense will not be coerced, and sensible writers will go on writing as sensibly as they know how.*

Rule 25 provides for expunging from nomenclature any name which is 'offensive (whether politically, morally, or by its irreverence).' This raises a question of great and probably insuperable difficulty; for we may at once ask, offensive to whom? In a certain sense, science is non-political, non-moral (a different thing from being immoral, of course), and non-reverent of anything but ascertained or ascertainable truth. In religion it is agnostic; in printed decencies it is usually found to side with decent people; in political lampooning it might display partisanship as a part of human nature, without great offence to propriety. It would seem to be a case which ordinary self-respect and regard for public opinion would suffice to regulate; yet we can hardly arbitrate it in its nomenclatural aspects. *Sivatherium* might be as offensive to a Hindu devotee of the cult whose god is invoked in the name of a fossil beast as any other blasphemy. Neither *Phallusia* nor *Ithyphallus* offends more than the translations of these terms would in a medical treatise, nor does *Clitoria* keep a manual of botany out of schools. While the drift and purport of Rule 25 are obvious and admirable, its enforcement to the extent of expunging any names but those quite outside the pale of public decency is probably impracticable. The way to deal with such things is to cut the perpetrator the first time he shows himself in society.

We have already protracted this review beyond usual limits, and must hasten to cloture. Several following rules bear upon correction of names, orthographically or zoologically. In the former regard they will be nugatory with those who hold to our Canon XL.; in the latter respect they will command the assent of some naturalists, but not of all. Rule 42 will, we imagine, be found decidedly objectionable, as will most of its corollaries; though some of the refinements regarding types may be specially serviceable in microlepidoptera, while less so or not so in zoology at large. In any event, Walsingham and Durrant have given us in this Code a notable contribution to the literature of the subject, which can be studied to advantage by every zoologist, perhaps by every botanist also. For ourselves, we are among a large number of naturalists who are fully convinced that the A. O. U. Code is, on the whole, by far the best one ever formulated. Our appreciation of its manifold good qualities and general utility leads us not unnaturally to set it up as a standard of excellence with which other codes are to be compared. That we are not blind to its defects is obvious from what has preceded; but it is our very conviction of its strength and worth which makes us feel free to express ourselves perhaps more forcibly regarding its blemishes than we should if we considered it a weak or tender thing that needed nursing. At the same time it were idle to consider our Code a faultless finality; no sensible man is going to be bound by it against his own convictions, and if naturalists are ever to be

*Dr. B. G. Wilder states, Journ. Comp. Neurology, vi., Dec., 1896, pub. Feb., 1897, p. 334: "No such attempt is known to me." Then he never knew the A. O. U. Code and Check List, which are an attempt to secure uniformity of nomenclature by the authority of a committee, "*quorum pars magna fui.*" But his criticism is excellent: "The very notion savors of ecclesiasticism rather than of science. At the most, individuals have set certain fashions, more or less commendable and permanent, while committees have made recommendations which even their own members may disregard when their information is increased or their views are modified."

blessed with such a thing as an infallible rule of faith and morals in matters of nomenclature that state of beatitude may be sought along the line of one of Lord Walsingham's suggestions: "All branches of zoological study should undoubtedly be represented on any committee entrusted with the task of drawing up rules for general guidance." Pending any final consummation, British ornithologists will no doubt continue to lean upon Strickland and the B. A. Code. American ornithologists, and most zoologists of this country, will stand by the A. O. U. Code; while doubtless the Merton Rules will be respected by most of those entomologists whose requirements are so ably met in this instance.

Elliott Coues.

As said above, the 'Merton Rules' agree in the main, or at least on most points of leading importance, with other recently promulgated Codes of Nomenclature; but they embrace many provisions, by no means all new, which are open to strenuous objection, on the ground that they seriously militate against the stability of names in zoology. Some of these have been pointed out in the foregoing review; others have been passed over leniently or quite unnoticed. On the other hand, some which we consider utterly objectionable have received approval.

Rule 5, for example, provides that "The same name may be used once only in the same grade, with the exception of special [specific] names, so long as they occur in different genera, and of subspecial [subspecific] names so long as they are subservient to different species." Since 'subspecial,' or subspecific, names are often applied to forms of uncertain status, and may be regarded either as species or subspecies by different writers, it is obvious that they should fall under the same rule as 'special,' or specific, names. Otherwise they are open to instability, according to whether the forms to which they may be applied may be regarded as 'species' or 'subspecies' by different authors.

Rule 19 divides 'invalid names, considered merely as words,' into three classes: (1) *Homonymous*, (2) *Homophonous*, and (3) *Synonymous*. 'Homophonous' words are 'words differently written, but indistinguishable in sound, applied to different conceptions.'

Rule 20 provides that "A name homophonous with a valid name is invalid, in accordance with the rule governing homonomy (Rule 5)." (See Rule 5, as quoted above.) This we regard as a pernicious innovation. Previous codes advise that in selecting new (especially generic) names those closely resembling previous names in orthography or sound be avoided; but this is the first instance, we believe, where they have been declared invalid. The objections to this rule are: (1) that scientific nomenclature belongs properly to written language, not to spoken language; (2) that whether a name is or is not too near in sound, when properly pronounced, to another earlier name must be largely a matter of opinion, respecting which authors must frequently disagree; this disagreement being necessarily a fruitful source of instability in names. It opens a loophole for the displacement of well-known names by new ones on the ground of personal opinion or preference, perhaps biased by the opportunities thus presented. In all languages there are almost innumerable homophonous words of radically different origin and meaning; why should they not be admissible in the language of science?

Rule 20 is thus in opposition to the intent of nearly all modern codes, which are designed to leave as little as possible open to the notoriously unsafe decision of personal judgment or option in matters of nomenclature.

According to Rule 21, which is an amplification of Rule 20, a name may be only 'so similar to a valid one as to be *almost homophonous*' to be rejected, which, of course, but emphasizes the objections already made to Rule 20.

It follows that Rule 22 is also objectionable (1) in that it provides that "a name wrongly written is invalid if, on legitimate correction, it becomes (either) homonymous or homophonous with a valid name;" and (2) in that it implies the right of emendation of names.

Yet Rule 23 permits, in the case of genera, the use of names which differ merely in 'a different sexual suffix,' as in cases like *Sciaphilus* and *Sciaphila*, in opposition to previous codes and the consensus of probably nine-tenths of present zoological writers. The case of *Picus* and *Pica* is obviously not parallel, inasmuch as these names have come down to us from pre-Linnæan classical writers, who employed them, not interchangeably, but as names for entirely distinct objects. The etymology of these words is admittedly unknown, but their use for centuries as distinct names, applied to totally unlike birds, seems sufficient reason for their use in modern systematic nomenclature, and for their adoption as distinct words, despite the accident of their similarity.*

Rule 24. "A name which involves a false proposition is invalid and may be changed." Note first the use of 'may' instead of *must*, leaving the enforcement of the rule optional, and thus opening a way to instability of names through mere personal opinion or preference. Note, secondly, that it opens the way to wholesale changes on trivial grounds. If a species of a certain genus has been named *minimus* or *major*, and a species is subsequently found that is smaller or larger, the names in question 'involve a false proposition.' And so in cases where names relate to color, as where a species is named *nigra*, or *purpurea* or *rubra*, and is not of the color implied; or in the case of species given geographical names that do not occur in the countries whose names they bear, or in other cases indicate portions of their range which are not characteristic of their distribution. For instance, a Mexican species may have been named *brasiliensis*, or a Peruvian species *cayennensis*, which do not occur respectively in Brazil or Cayenne. This class of cases, as everyone knows, is legion. To avoid the adoption of a rule necessitating such sweeping subversion of names we can well afford to endure the isolated case of a name like *cafer* for a North American woodpecker! *Ludovicianus*, as applied to one of our tanagers, now 'involves a false proposition,' since the species does not occur in Louisiana. There is, in fact, every shade of falseness in names, from a time-honored *Paradisea apoda* to such as involve a falsity so slight as to be of questionable importance in the mind of even the greatest stickler for nomenclatural veracity.

Rule 27 is the corollary of the principle laid down in the 'introduction' regarding the 'guarding' of the 'actual work and *intention* of an author,' quoted, and so emphatically commended in the foregoing review of the 'Merton Rules.' The Rule is: "The right of an author to correct antecedent work is undoubted, provided always that in making such corrections the *intentions* of his predecessors be respected, unless proved to be erroneous." This is an innovation fraught with the gravest possibilities for mischief. Heretofore it has been deemed to be the only safe course to take an author's work as he left it, without attempting to

*Since this was written this point has been touched upon in this journal (SCIENCE, N. S., Vol. V, No. 126, May 28, 1897, p. 847) by Dr. Stejneger, who says *Picus* and *Pica* "are distinct and separate Latin classical names for widely different birds, though the philological root of the two words is probably the same."

guess at what he intended to do, even if in rare cases we may be compelled to ignore an obvious intent. The trouble here is that when the bars are let down there is no limit to the license authors may take, for one reason or another. The case of *Dendragapus*, cited above as an illustration, shows to what strange conclusions such license may lead. It is purely a matter of opinion whether *obscurus* and *canadensis* are generically separable. For those who think they are not, the only course open is to treat them as members of one genus, and to take the oldest tenable name available for the group, in accordance with a fixed principle of nomenclature especially provided for such emergencies. The first tenable name happens in this case to be *Dendragapus*. If this is 'puerile, unscientific and immoral,' there is no help for it, even under the 'Merton Rules.' All it amounts to is a difference of opinion between ornithologists as to the value of the differences which distinguish the two species *obscurus* and *canadensis*.

Rule 30 provides, among other things, that "an orthographical correction may be made by emending a name wrongly formed." This is not an innovation, as the same provision is found in other codes. But it is diametrically opposed to Canon XL. of the A. O. U. Code, which provides that "the original orthography of a name must be rigidly preserved, unless a typographical error is evident." Sound and weighty reasons are given in the A. O. U. Code for its adoption; it is in accord with the practice and advice of many eminent authorities—some of whom lived before the A. O. U. Code was thought of, and were scholars as well as naturalists; and, while still repellant to some, it is obviously gaining ground, as there is an increasing number of writers who consider fixity of names as of higher importance than the correction of grammatical or philological imperfections in their construction. Not only is this view shared very generally by American mammalogists, but we have recently found that it has strenuous advocates among eminent specialists in other departments of zoology, notably in entomology.

When we consider that purists and classicists are often at loggerheads among themselves over the emendation of a name; that names are often 'emended' out of all resemblance to their original form and become, to all intents and purposes, new words; that, when less modified, the initial letter is often the part affected, and that through this change the name takes a new place in all indexes and in all alphabetic lists where it appears, resulting in an inconvenience of serious magnitude—it seems far the lesser of two evils to put up with here and there an orthographic abomination than to sacrifice stability of nomenclature to philological refinement. Carelessness or ignorance in the construction of names is to be deprecated and frowned upon, as it is in the remarks under the much abused Canon XL. of the A. O. U. Code, which, therefore, does not place 'a premium on ignorance or carelessness,' but simply chooses by far the lesser of two evils. It conforms to the whole spirit of the Code, which aims at stability in names, and the elimination of every element of instability that may arise from personal preferences in matters where opinions must inevitably differ to a greater or less extent. Even my fellow critic of the Merton Rules cannot agree with Lord Walsingham as to the extent emendation is allowable, as in the above-cited case of changing *cretidactylus* to *gypsodatylus*.

As to the case of *Leptotila*, dwelt on at length above, if 'tales out of school' were admissible, the inside history of its adoption by the committee would afford an amusing commentary on some of the remarks above made. Suffice it to say that

as *Leptotila* was repeatedly used by Swainson, and thus with obvious intent, it could not be ruled out as an 'evident typograhical error' for *Leptoptila*, and so was accepted as simply a name, and therefore available under the A. O. U. maxim, "A name is only a name and has no necessary meaning;" or, to cite the B. A. Code of 1842, "In truth, it matters not in the least by what conventional sound we agree to designate an individual object, provided the sign to be employed be stamped with such an authority as will suffice to make it pass current." It is, therefore, entirely thrown out of the category of such cases as *Fregetta* and *Fregata*, discussed above.

It certainly is to be hoped that all sensible writers will go on writing as 'sensibly as they know how;' but in the above remarks on *cafer* and *Leptotila*—ostensibly anent the 'Merton Rules,' but really in ridicule of the A. O. U. Code—it is evident that not all of the 'puerility' is on the side of the supporters of Canon XL.

Nos. 34–37 of the Merton Rules call for no comment, being in essential conformity to current usage. We must dissent, however, from Rule 38 in so far as it relates to 'co-types,' this part being to the effect that when a species is 'described from more than one specimen, no single one being selected as the type,' the 'type' in this case is 'the sum of the co-types.' The position here taken seems so obviously unwarranted as to hardly merit discussion.

Rules 42–48, on the restriction of genera, are refinements of existing rules relating to this subject, treating the matter in detail on lines already for the most part generally approved.

Rule 49 provides a most cumbersome way of designating subspecies. Rules 50 and 51 relate respectively to the use of signs and methods of citation, the latter formulating practices already more or less in vogue.

As already said, the 'Merton Rules' are in the main in accord with other advanced modern rules and usages; the innovations, as noted above, are for the most part positively mischievous, from the standpoint of fixity in names; the adoption of the tenth edition of Linnæus's 'Systema Naturæ' we regard as the one especially commendable feature of this new code, only so, however, on account of its geographical origin, since all recently promulgated Codes take this date as the starting point for the law of priority.

J. A. ALLEN.

THE ORIGIN OF GREEN RIVER.

IN his Current Notes on Physiography in No. 121 (April 23d) of this JOURNAL, Professor Davis, under the heading: 'Is Green River antecedent to the Uinta Mountains?' remarks that this question is not closed, as had been assumed by Mr. J. D. Irving in his paper on 'the Brown's Park beds of Utah,' and further that it does not appear clear from the latter's statements whether he considers it to be a superposed river, as maintained by me, or antecedent, as stated by Powell. He very pertinently remarks that it is remarkable, considering how frequently the Green is referred to as an antecedent river, that so little attention is given to the difficulties that such origin involves. Long before the appearance of the two textbooks he quotes (Tarr and Scott), LeConte and Geikie had each referred to it as antecedent and illustrating the slow uplift of mountain ranges, in apparent unconsciousness that any other view is possible. Suess, on the other hand, in his exceedingly careful review (Antlitz der Erde, I., p. 736) of the structure of this region, adopted my view without any reference to that of Powell.

In Powell's original publication (Exploration of the Colorado river of the West, p. 153) he makes no mention of the struc-

tural difficulties the river might have to contend with, but contents himself with the simple statement: "The river had the right of way; in other words, it was running ere the mountains were formed; not before the rocks of which the mountains are composed were deposited, but before the formations were folded so as to make a mountain range." Later he remarks: "I reserve the subject for a more full discussion in my report on the geology of the Uinta mountains." In this report, however, I had been able to find no mention of the subject whatever, and I had assumed that upon further study he had found the difficulties in the way of his theory too great to be explained away.

My study of the region was made in the summer of 1871. Powell continued his during the years 1874 and 1875, after, at his request, I had explained to him my views as to the structure of the range.

Upon the following facts with regard to its geology we are both agreed. The uplift of the Uinta Arch commenced at the close of the Cretaceous. During Tertiary times there were deposited in the lakes, which washed either flank of the range, not less than 8,000 feet of sediments that were derived, in part, at least, from the degradation of that Arch.

Now, as my map shows, these Tertiary beds, overlapping in a nearly horizontal position the upturned and truncated edges of the various formations composing the original arch, reach altitudes of 9,000 and 10,000 feet at various points along either flank of the western and higher portion of the range. The eastern portion of the range, through which the peculiarly winding cañons of the Green River have been cut, has an elevation of only 7,500 to 8,500 feet, a few of the higher points reaching 9,000 feet, and in one case 9,297 feet.

According to Powell's theory, however, the river had determined its course before the uplift of the arch, and has continued to occupy the same bed to the present day. "The principal cañons through the mountains," he says, "had been carved nearly to their present depth before the last of these sediments were deposited."

What, then, became of the river while these 8,000 feet of Tertiary sediments were being deposited? It could hardly have continued its course at the bottom of the Tertiary lakes while the sediments were depositing. But if it ceased to flow during this time its bed must have been filled with sediments as well as the rest of the country, and when the lakes were finally drained, it is hardly conceivable that, in redetermining its course across the 150 miles of Tertiary beds on the north side of the range, it should have attacked the flanks of the Uinta range, themselves partially buried, at exactly the same point it had entered before.

There are many other features that require explanation before Powell's hypothesis can be accepted, one of which Professor Davis has pointed out in the Brown's Park depression, a longitudinal valley 40 miles long and 5 to 6 miles wide, open only at its eastern end, and nearly in the axis of the range. It is supposed to have been formed by engulfment, and has twice been occupied by Tertiary waters, once in Eocene, and once in Miocene or later times.

How the river kept its course (which three times wantonly leaves the open valley to cut cañons in its hard walls) through all these vicisitudes, would seem to require a more direct explanation than that "The river preserved its level, but the mountains were lifted up, as the saw revolves on a fixed pivot, while the log through which it cuts is moved along."

Inasmuch as the promised discussion did not appear, I have recently asked Major Powell to explain to me his conception of how these things could have been accom-

plished, but he says it is so long ago he no longer remembers the course of reasoning he followed at the time.

I assume that Capt. C. E. Dutton, who at Powell's request took up and completed the latter's Colorado cañon geology, is likely to have voiced his matured opinion on this point. In his paper on the Grand Cañon (2d Ann. Rep. Director U. S. Geol. Survey, p. 62), in treating of the persistence of rivers, Dutton gives a most graphic description of the course of the Green river in its passage through the Uinta mountains. In spite of the fact that he places Horseshoe Cañon on the south instead of on the north flank of the mountains, it is evident that he must have read Powell's description, for he uses not only his metaphor about the 'right of way,' but also the simile of the saw preserving its position while the log moves. Whether consciously or not, however, he certainly does not agree with Powell's hypothesis, for he says in conclusion: "What then did determine the situations of the present drainage channels? The answer is that they were determined by the configuration of the surface existing at or very soon after the epoch of emergence. Then, surely, the water courses ran in conformity with the surface of the uppermost (Tertiary) stratum."

Dutton elsewhere states more definitely that the course of the Green or Colorado river south of the Uinta mountains was determined at the close of the Eocene. If this is correct, I was probably wrong in assuming that the Green river first found its way across the Uinta mountains after the Wyoming (Bishop's Mt.) conglomerate had been deposited, because I found undisturbed remnants of this formation on either side of the river, both on the north and south flanks of the mountains and at such elevations that, if the beds were continued across the intermediate country on the same level, they would completely cover that portion of the mountains through which the Green river now runs. I have for a long time been hoping and still hope that some other geologist may make a more thorough examination than I was able to at that time, and determine the nature and extent of this singular formation, which has never been satisfactorily accounted for. Whatever may be the outcome of such an examination, it would seem proper that the antecedent origin of this river should be held in abeyance until some positive evidence of it can be furnished.

S. F. EMMONS.

U. S. GEOLOGICAL SURVEY.

ZOOLOGICAL NOTES.

THE SCIENTIFIC NAME OF THE VISCACHA.

ONE of the best known mammals of the pampas of the Argentine Republic is the viscacha, now usually called *Lagostomus trichodactylus*. Unfortunately this name proves to be untenable, but in order to show that such is the case it will be necessary to refer briefly to the history of the species. The animal was first described in 1801, by Azara, who considered it identical with *Cavia acuschy* of Gmelin, which is now known to be an entirely distinct species. Rafinesque, in 1815,* proposed the genus *Viscacia*, apparently without description, so that his name is not entitled to recognition. One year previous, in 1814, according to Waterhouse (Nat. Hist. Mamm., Rodentia, 1848, p. 213), a living viscacha was placed on exhibition in London,† where it was examined by Blainville and Cuvier. Blainville soon after described the species as *Dipus maximus*.‡ Some years later the same animal came into the possession of Brookes, a member of the Linnæan Society of London, who gave a full description both of its

* Analyse de la Nature, 1815, p. 56.

† Burmeister states that there were two.

‡ Nouv. Dict. d'Hist. Nat., nouv. éd., XIII., 1817, pp. 117–119.

skeleton and of its external characters in a paper read before the Society on June 3 and 17, 1828.* Brookes recognized the fact that the viscacha belonged to a distinct genus which he named *Lagostomus*. He also changed Blainville's specific name *maximus* to *trichodactylus* on the ground that it became inappropriate in connection with a genus represented by only one species. Authors who have adopted *Lagostomus trichodactylus* have reduced *Dipus maximus* and other subsequent names to synonymy, but, almost without exception, have overlooked one of the most important references to the species.

In 1824 Schinz began the publication of his 'Naturgeschichte und Abbildungen der Saugethiere,' and on page 244 of this work gave a full description of the viscacha, calling it *Vizcacia pamparum*. A comparison of the title pages of this work (1824) and of volume XVI. of the Transactions of the Linnæan Society (1828) seems to indicate that *Vizcacia pamparum* Schinz has 4 years priority over *Lagostomus trichodactylus* Brookes. Although Schinz's name was undoubtedly published first, its actual date of publication is uncertain. Schinz's Naturgeschichte appeared in 29 Hefte, at intervals from 1824 to 1828, and, as the description of the viscacha is inserted near the middle of the book, it was probably not published before 1825 or 1826. I have been unable thus far to ascertain the dates of publication of the separate parts of the Naturgeschichte, but in the copy examined is a notice to subscribers, printed for distribution with the 29th Heft, and dated February 28, 1828, stating that this is the concluding part of the volume. Schinz's work was evidently completed several months before Brookes' paper was even read, and possibly a year before it was actually published, if we accept the statement in Oken's Isis (1830, p. 906) that the latter appeared in 1829.

Vizcacia therefore, is, probably not less than 2 years earlier than *Lagostomus*, and, as the objection to Blainville's specific name would not be considered valid by modern zoologists, the species should stand *Vizcacia maxima* (Blainville).

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Washington, D. C.

* Trans. Linn. Soc. London, XVI., pt. I, pp. 95–104, 1 plate.

CURRENT NOTES ON PHYSIOGRAPHY.

Bosphorus, Rhine and Hudson.

Philippson's 'Geologisch-Geographische Reiseskizzen aus dem Orient' (Sitzungsber. Niederrhein. Gesellsch., Bonn, 1897) include, among many other items of interest, a clear account of the Bosphorus as a partly drowned valley incised in an uplifted peneplain of deformed Devonian strata. Viewed from the summit of Bulgurlu, a low quartzite monadnock that surmounts the upland east of Skutari, the peneplain, only here and there interrupted by rounded knobs and ridges, is seen to ascend slowly northward, and then to rise in a marginal ridge of harder strata along the border of the Black Sea. The upland is generally unoccupied, being rather barren, in part from natural infertility, in part from exhaustion of the soil such as characterizes the vicinity of nearly all the great millenial cities of the Mediterranean. The Bosphorus trench has a winding course, the water surface being 200–300 met. beneath the upland. The water is generally 50 met. deep, but becomes shallower near Constantinople, as if by the washings and waste from that old city. Philippson justly compares the gorge of the Bosphorus to that of the Rhine. A still closer analogy might be found with the gorge of the Hudson, since the latter is a drowned river, deep and navigable to large vessels, while the Rhine is a running river, comparatively shallow in the gorge and interrupted by rapids and islands.

Speaking of the Rhine, mention may be made of a geological essay by Rothpletz, of Munich (Das Rheinthal unterhalb Bingen, Jahrb. k. preuss. geol. Landesanst., 1896, 10–39), in which it is shown that for about four kil. in the head of the gorge it follows close upon a narrow block between two sub-parallel faults of considerable dislocation. Although no vertical movement is proved for the block, the author concludes that there was a 'direct genetic connection' between the dislocation and the origin of the gorge, for similar disturbances seem to be wanting in the uplands (Taunus) further east. It may, however, be objected that until the date of the dislocation is shown to be recent, and until the movement of the block is shown to be downward, it is premature to make too close an association of these two phenomena. If, as is very possible, the dislocation is ancient, all sorts of happenings may have intervened before the river gained its present course; and among the factors that determined its location, the dislocation may have had a relatively subordinate rank. To place ancient structural disturbances in direct genetic connection with the location of a very modern river gorge suggests that a great many intermediate processes are overlooked.

THE GEOGRAPHY OF BRITTANY.

PROFESSOR CHARLES BARROIS, of Lille, long occupied on the intricate geology of Brittany, has recently presented a summary of its geographical divisions, with a special chapter on the influence of its physiography on its advance in civilization (Ann. de Geogr., VI., 1897, 23–64, map). The region is composed of ancient rocks, strongly folded and dislocated on east and west lines, whose trend diverges inland. To-day the surface is that of a dissected peneplain, with even skylines and a gradual descent to the north and south, the former steeper than the latter. The author believes that the ancient mountains indicated by the structure have been removed by subaërial weathering and washing, not by marine erosion. The existing longitudinal drainage lines do not occupy synclinal axes, but follow open valleys or *combes* along the strike of weak strata, after the fashion of subsequent streams. The transverse rivers, by which the inner belts of low ground are drained, cross the uplands in sharp trenches or *cluses*, across which viaducts and suspension bridges are carried above the steeples of the valley villages. These transverse streams are called *consequent*, as if determined by the lateral slopes of the original deformation (post-Carboniferous); their possible origin by superposition from a Mesozoic cover, now stripped away, is not explicitly considered. In this respect the suggested comparison of the drainage on the ancient folded structure of Brittany with the rivers of the relatively young and simple dome of the English Weald does not seem to be fully warranted.

INSEQUENT RIVERS.

INITIAL land forms, due to uplift, deformation and other non-destructive processes, are gradually carried through a systematic series of sequential changes, chiefly under the action of streams, sometimes under the action of waves, wind or ice. Omitting antecedent streams from present consideration, the first streams to be developed on an initial surface are the *consequent* streams, so named by Powell. If the valley sides of the consequent streams lay bare a disordered structure of variable resistance, gullies and ravines will grow along the guidance of the weaker structures, and thus *subsequent* streams will be developed by headward erosion; the term *subsequent* being first used in this connection by Jukes, although not in a technical sense. It sometimes happens that no guiding weak structures are revealed on the consequent

valley sides; such being the case when the rocks are either homogenous or horizontally stratified. Then the side streams, growing headwards, are accidentally located; and streams of this class have been called *autogenetic* by McGee. *Insequent* may prove to be a more satisfactory name for such streams, as it is of the same etymological family as *consequent*, *subsequent* and *obsequent;* the latter being defined in the *London Geographical Journal*, V., 1895, 134. As *insequent* has proved servicable in my lectures during the past winter, it is now submitted for trial by others.

NEW TERMS IN GEOLOGY AND GEOGRAPHY.

With a protest against the introduction of new terms in geology as a rallying cry, a good friend of all geologists has unfurled his banner to the breeze in a recent number of Science, as if inviting those who are of his opinion to enlist in a crusade against a threatened inundation of scientific verbiage. Those holding other opinions may prefer to enlist in friendly opposition in another camp, under the leadership of a geologist who said some years ago that he felt he had accomplished a good piece of work by introducing a new name for certain deep-seated igneous structures. For one, I dissent entirely from the dictum that new terms are 'evidently nothing more than a useless incumbrance to the science' of geology. New terms are an absolute necessity in any science that is advancing. Useless terms are of course objectionable, but who shall say which are the useless ones? New things and new ideas must have new names. There is too much good new wine to be held in old bottles. 'The common run of educated people in this country' are entirely excusable if they know nothing of such new terms as monadnock and peneplain, or of such older terms as novaculite and bauxite; for their ignorance and their opinions are irrelevant in technical matters. The investigator and the specialist must be left as free to name their conclusions as to reach them; and they, much better than any one else, can judge of the need of new names. By all means, let them be cautious and avoid unnecessary names; but unless they can number their new finds, as astronomers number new asteroids and new comets, they must name them. Having invented a new name, they may well let it take its chances in the struggle for existence. If it prove acceptable to workers in its field, it will take root and flourish; if not, it will soon wither away and be seen no more. As far as new terms in physiography are concerned, I have had a good share of amusement in watching the fate of certain words that have appeared in recent years. Some have survived and some have perished. Among several that appear to be destined to survive, although not generally used at present, let me commend 'subsequent,' 'adjusted' and 'graded' (ordinary words used in a technical sense), 'obsequent' and 'insequent' (new-made words of English form), and 'doab' and 'cuesta' (imported foreign terms), to the attention of those who maintain that new terms are evidently nothing more than a useless incumbrance to a science. It will be interesting to note the standing of these words ten or twenty years hence. The amount of attention given then or now by teachers and students to physiography—or geomorphy, as some neologists would call it—may be measured by the terseness and precision with which they express the ideas or things represented by these words and their fellows.

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CURRENT NOTES ON ANTHROPOLOGY.

CENTRAL AMERICAN CARIBS.

The Caribs, who spread so widely over South America, never reached any part of

North or Central America in pre-Columbian times, so far as we know. About a hundred years ago the English deported a large number of them from the island of St. Vincent to the island of Ruatan, in the bay of Hondnras. In the century which has since elapsed they have spread extensively over Central America, retaining largely their language and traits.

An interesting sketch of them is given by Dr. Carl Sapper, in the 'Internationales Archiv für Ethnographie,' Bd. X. The changes in their dialect by loss of forms and the introduction of new words from various European tongues are numerous, but its affinity to the Carib of the islands is unmistakable. Their arts also are South American.

The latter feature is further illustrated by an article from the pen of the editor of the *Archiv*, Dr. J. D. E. Schmeltz, on the utensils of the Caribs of Surinam, Dutch Guiana. It has an especial interest as tracing the development of the bird-motive in Caribbean art products. Several colored plates explain to the eye the descriptions in the text.

THE ANTHROPOZOIC FORMATION.

THE strata in geologic deposits which include the remains of man are called 'the anthropozoic formation.' To divide this accurately, with reference to sequence of time, on the one hand, and development of culture, on the other, is a leading task of the anthropologist. Professor Woldrich, of Prague, has proposed a scheme for the area of central Europe, which is published in the 'Centralblatt für Anthropologie,' 1897, Heft 2. It is useful as a general standard and it merits an epitomized reproduction here.

The Anthropozoic Formation.

I. The Diluvial Epoch.
  A. Palæolithic period.
    1. Preglacial.
    2. Glacial and Interglacial.
    3. Post-glacial.
  B. Mesolithic period.
II. The Alluvial Epoch.
  C. Neolithic period.
    1. Old or atrymolithic (no bored stones).
    2. Middle or trymolithic (bored stones begin).
    3. Late, with the stones dressed to art shapes.
  D. Metallic Period.
    1. Bronze age.
    2. Iron age.
    3. Protohistoric age.
    4. Historic age.

The author adds numerous examples of these several divisions from the layers of central Europe, and further specifications of their characteristics. He dismisses the 'hiatus' between the palæolithic and neolithic maintained by some archæologists, believing the development to have been gradual and uniform.

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NOTES ON INORGANIC CHEMISTRY.

THE statement is frequently found in the text-books that when platinum-silver alloys are treated with nitric acid a considerable quantity (5–9%) of platinum follows the silver into solution. In the Proceedings of the Chemical Society Mr. John Spiller gives the results of experiments on such alloys, containing from 0.25 to 12% platinum. In dilute nitric acid (1.2 sp. gr.) only about 0.25% of platinum was dissolved; with ordinary concentrated acid (1.42 sp. gr.) the maximum platinum dissolved was 1.25% and the average 1%. It thus appears that the ordinary statement is incorrect.

PROFESSOR RAMSAY recently read a paper before the Royal Society, detailing a series of experiments which show that helium

and argon do not pass through red-hot septa of platinum, palladium or iron. It is well known that hydrogen passes with great ease through such septa, either owing to solubility in the metal or the formation of easily decomposable compounds. Professor Ramsay's experiments imply the inability of helium or argon to form any even unstable compound with these metals or to dissolve in them at red-heat, another evidence of the inertness of these gases.

The following from the *Chemical News* deserves to be quoted in full: "Atomic Models (Patent No. 1999, 1897). A patent has recently been granted to Mr. Frederick George Edwards, of London, by which the government affords protection to his idea that all atoms can be represented by varying numbers of tetrahedrons. The germ of the idea appears to be that as there are about seventy elements known to chemists, and that the tetrahedrons can be grouped together in as many as seventy different ways, the latter can illustrate the former. This is the idea; the practice, the inventor shows, is not so simple. For instance, he says: 'Regular tetrahedrons do not fit exactly, but each tetrahedron is so nearly regular that it may be supposed that each of the elements were (*sic*) created from regular tetrahedrons in a plastic condition.' This strikes us as a beautiful example of inventing facts to fit a theory. We are glad to find that Mr. Edwards has not patented atoms *per se*, but merely the form he thinks they take, together with a few names of elements, hitherto undiscovered, but predicted by him. These are: icosagon, atomic weight, 10; *x*-odine, atomic weight, 215; and zadmium, atomic weight, 245. It will be interesting to watch the action for infringement of patent which will result if any chemist engaged in research should have the temerity or the misfortune to discover either of these predicted elements. A lithographed diagram at the end gives the *shape* of thirty-two elements, with atomic weights made to fit; we are sorry to have to record the fact that many of these atomic weights are wrong, but then so probably are the shapes."

The *Chemisches Centralblatt* gives an abstract of a lecture by L. Wenghöffer, of Berlin, on the incandescent (Welsbach) gas-light. Among many points we note the following: Auer von Welsbach is not the original discoverer of the incandescent light, but deserves the credit of having brought the light to its present perfection and made it an industrial success. The oxids for the mantles must be chosen with great care, as well as their proportions, to attain the maximum brilliancy. The best results are reached with about 99% thoria and 1% ceria. The presence of any other of the rare earths does not improve the light, and a greater or lesser proportion of ceria is detrimental. 'Russium' nitrate, the use of which has been recommended in the place of cerium, proved in one case to be a mixture of thorium nitrate with much cerium nitrate, and in another case to be a mixture of cerium and ammonium nitrates. 'Lucium' oxid is impure yttria. The best source of thoria is monazite sand, and the price of thorium nitrate has fallen from $500 per kilo at the opening of 1895 to $5 or less at present. The cause of the low price is competition, and there is no probability that the supply will diminish. The source of the cerium is the by-product of the thorium manufacture, and the price of its nitrate is steady at about $50 per kilo. The bath for saturating the mantle is a 30% aqueous solution of thorium and cerium nitrates in the proportion of 99 : 1.

From experiments on the action of ammonia on mercurous iodid, Maurice François, in the *Journal de pharmacie et chimie*, is led to the belief that the dark product formed by the action of ammonia on calomel is not a mercuro-ammonium compound, as has been

supposed, but a mixture of the colorless mercuri-ammonium compound with very finely divided metallic mercury.

N. TARUGI, who introduced the use of thioacetic acid as a substitute for hydrogen sulfid in the laboratory, has, in the *Gazzetta chimica italiana*, a study of the action of thioacetic acid on salts of bismuth. The thioacetate of bismuth is decomposed by a small amount of water with the formation of a thiobasic salt $(CH_3COS)_3$ BiS, and this on treatment with sulfuric acid gives a sulfate $(CH_3COS)_2$ Bi SO_4, and on treatment with iodin the corresponding iodid. These new compounds are of interest as being the first compounds of quintivalent bismuth in the marsh-gas series.

J. L. H.

SCIENTIFIC NOTES AND NEWS.

THE Senate has agreed to the provisions in the tariff bill admitting free of duty books in print more than twenty years, books in foreign languages and such as are devoted to scientific research, and books and scientific instruments imported for public and educational institutions.

PERHAPS the most noteworthy additions to the United States National Museum during recent years have been the rich collections of pre-historic pottery, made in the pueblo region, during the last two seasons, by Dr. J. Walter Fewkes, of the Bureau of American Ethnology. The success attending his operations in the past has led to a provision for continuing the work, and he has recently set out to the field for the third time. His design is to survey and excavate the ruins known among the Indians as Kintiel, near Navajo Springs, Arizona. He is accompanied by Dr. Walter Hough, of the United States National Museum.

THE keen appreciation of the importance of research on the part of Secretary Wilson has already given fresh impetus to various lines of scientific work in the Department of Agriculture. The more important operations are carried forward without change of personnel; it seems to be the policy to maintain and strengthen the bureaus built up through the zeal and ability of well-known scientific men; and, at the same time, scientific character is given to certain of the lines of work hitherto regarded as administrative. One of the recent changes is the appointment of Mr. John Hyde as Statistician of the Department. Mr. Hyde became widely known through his connection with the Eleventh Census, and he has more recently been known in scientific circles as editor of the *National Geographic Magazine* and as a writer on political economy.

MAJOR J. W. POWELL is on the coast of Maine, engaged in researches concerning shell mounds, in the interest of the Bureau of American Ethnology.

PROFESSOR SOUILLARD, astronomer at Lille, has been elected a corresponding member of the Paris Academy of Sciences.

DUBLIN University has conferred the degree of D. Sc., on Professor William Ramsay, Major P. A. M'Mahon, D.D., and Professor Wilhelm His, of Leipzig.

DR. RUDOLPH LEUCKART and Dr. Karl Neumann have been made Knights of the Prussian Order of Merit in Science and Art.

THE Royal College of Surgeons of England has conferred the John Tomes prize on Mr. C. F. Tomes, F. R. S.

THE *Naturwissenschaftliche Rundschau* states that, at a recent meeting of the Academy of Sciences of Vienna, Ritter v. Arneth was reëlected President; Dr. Suess, Vice-President; Professor Huber, General Secretary, and Dr. Hann, Secretary of the Mathematico-Physical Section. Dr. Gautsch v. Frankenthurm and Dr. Exner, of Innsbruck, were elected corresponding members; Lord Lister, honorary foreign member; and Dr. Vogel, of Pottsdam; Herr Karpinsky, Director of the Geological Institute of St. Petersburg; Dr. Gegenbauer, of Heidelberg, and Herr Brioschi, of Milan, corresponding members.

M. LE GÉNÉRAL DE TILLO announced at a recent meeting of the Paris Academy of Sciences that the sum of 25,000 francs had been subscribed in Russia to the Lavoisier monument fund.

M. FALGUIÈRES has been commissioned to design the Pasteur statue to be erected in Paris, for which 297,000 francs has been subscribed.

THE United States Civil Service Commission will hold an examination on July 20th to establish a register of eligibles for the position of industries in the Indian school service. The subjects of the examination will be penmanship, spelling, methods of manual training, arithmetic, geometry, geography, industrial economy, mechanical or industrial drawing (one optional), free-hand drawing and physics. There is a vacancy at the Indian school at Perris, Cal., the salary of which is $840 per annum.

WE regret to record the deaths of DeVolson Wood, professor of mechanical engineering at the Stevens Institute of Technology, Hoboken, New Jersey, on June 27th, at the age of sixty-five years; of Professor Julius von Sachs, on May 29th, at the age of sixty-five years; of the Rev. Alexander Freeman, on June 12th, at the age of fifty-eight years. Mr. Freeman was at one time deputy for the Plumian professorship of astronomy at Cambridge, and was a fellow of the Royal Astronomical, Mathematical and Physical Society; of Dr. Peter Von Tunner, at the age of eighty-nine. Dr. Von Tunner founded, in 1840, the school of mines at Leoben, in Styria, and was the author of many contributions on the metallurgy of iron and steel.

THE London *Times* reports that Dr. Wölfert and an assistant named Knabe were killed on June 12th while making trial of a navigable balloon invented by Dr. Wölfert. The balloon was being exhibited at Berlin before the officers of the ballooning section of the army, when the gas was ignited by the benzine used in the motor.

COMMISSIONER HERRMANN, of the General Land Office, has, it is said, submitted to the Secretary of the Interior a report regarding the rules and regulations for the government of the forest reserves, with special reference to the parts available for mining, grazing and agriculture.

IT is said that the Legislatures of the States of New York and New Jersey will be asked at their next sessions to provide for the building of a broad roadway 15 miles long at the base of the Palisades and to protect them from injury by quarrying.

WE are glad to learn that on June 19th a Zoological Club was organized at Springfield with nineteen members. The first officers are: President, W. W. Colburn; Secretary, Miss M. A. Young; Treasurer, Miss L. I. Mattoon; and Dr. George Dimmock and Miss M. A. Booth, additional members of the Executive Committee.

THE Park Board of New York City has adopted the plans for the buildings of the new Botanical Gardens in Bronx Park, as modified by the directors in accordance with the advice of a committee consisting of Professor Sargent and Messrs. Olmsted, Hastings and Parsons.

THE library of Columbia University has received from an anonymous donor 387 books, valued at $6,000. These include a number of valuable works in natural history, such as Audubon's Quadrupeds, Sepp's Nederland'sche Insekten, Gould's Humming Birds and Levaillant's Oiseaux d'Afrique.

IT is stated in *Die Natur* that Dr. O. Zacharias reported at a recent meeting of the Saxon Fisheries Association that the Prussian government would, after October of next year, give assistance to the biological station at Plön.

THE Boston *Transcript* states that an expedition under the charge of Mr. C. M. Harris, of Angusta, Me., has sailed from San Francisco for the Galapagos Islands, with a view to studying their fauna and flora. It is said that Messrs. E. D. Hall, of Dartmouth; F. P. Duowle, of Providence, and R. I. Beck and R. H. Beck, of California, are members of the expedition, and that the expenses are defrayed by Mr. Walter Rothschild.

THE Australasian Association for the Advancement of Science will, as we have already announced, hold its seventh session at Sydney, commencing January 6, 1898. Professor A. Liversidge is the President-elect. The President and officers of the several sections are as follows:

Section A—Astronomy, Mathematics and Physics. President, R. L. J. Ellery, C.M.G., F.R.S.; Secre-

taries, Professor A. Threlfall, M.A., J. Arthur Pollock, B.Sc.

Section B—Chemistry. President, T. C. Cloud, A.R.S.M., F.I.C., F.C.S.; Secretary, W. M. Hamlet, F.I.C., F.C.

Section C—Geology and Mineralogy. President, Captain F. W. Hutton, F.R.S., F.G.S.; Secretaries, Professor T. W. E. David, B.A., F.G.S., E. F. Pittman, A.R.S.M., F.G.S.

Section D—Biology. President, Professor T. J. Parker, D.Sc., F.R.S.; Secretaries, Professor W. A. Haswell, M.A., D.Sc., F.L.S., J. H. Maiden, F.C.S., F.L.S.

Section E—Geography. President, to be appointed. Secretary, H. S. W. Crummer, Secretary Royal Geographical Society of Australasia.

Section F—Ethnology and Anthropology. President, A. W. Howitt, F.G.S.; Secretary, John Fraser, B.A., LL.D.

Section G—Economic Science and Agriculture. President, R. M. Johnson, F.L.S.; Secretaries, R. R. Garran, B.A., F. B. Guthrie, F.C.S.

Section H—Engineering and Architecture. President, H. C. Stanley, M.I.C.E.; Secretaries, J. W. Grimshaw, M.I.C.E., M.I.M.E., H. C. Kent, M. A.

Section I—Sanitary Science and Hygiene. President, Hon. Allan Campbell, M.L.C., L.R.C.P.; Secretary, Dr. F. Tidswell, M.B.

Section J—Mental Science and Education. President, John Shirley, B.Sc.; Secretary, Professor Francis Anderson, M.A.

THE business meeting of the British Medical Association will be held in London on July 27th, to discuss the reports of the Council and of the various committees, and will then adjourn for the meeting at Montreal, beginning on August 31st. The meeting at Montreal, the first to be held outside the United Kingdom, promises to be very successful. The Dominion government has voted a sum of £1,000 to the reception fund, the government of the Province of Quebec £500, and other public bodies have been equally generous. The *British Medical Journal* has published an extended list of those who have signified their intention to be present, including the names of many eminent British physicians and men of science.

THE current number of *Nature* contains the first of a series of articles by the editor, Professor Norman J. Lockyer, on the approaching total eclipse of the sun. It appears that the weather prospects of the eclipse to be observed in India on January 22d of next year, are extremely favorable. The meteorological reporter to the government of India states that the chances of any given day in January being rainy in Konkan is less than one in one hundred and fifty. The Joint Committee of the Royal and Royal Astronomical Societies have determined to send out three parties to observe, one on the coast and two inland, at stations to be subsequently decided upon. It has been arranged that the party from the Solar Physics Observatory will occupy the coast station if the Admiralty can grant the use of a man-of-war to allow an attempt to be made to repeat the *Volage* programme of 1896. In this case the station will possibly be the old fort at Viziadurg. The land parties, which will include the Astronomer Royal, Professor Turner and Mr. Newall, representing the Observatories of Greenwich, Oxford and Cambridge, together with Dr. Common and Captain Hills, occupy stations near the central line on the railways shown on the map (Fig. 1).

IT is reported in the daily papers that Dr. David Starr Jordan stated before leaving for Alaska that, as the British government has not come to satisfactory terms with the United States for the protection of fur seals in Bering Sea, the United States will begin this summer, through the Fur Seal Commission, the work of branding the female seals in the Pribilof Islands. This will spoil the skins of branded seals and stop pelagic sealing by making it unprofitable. One of Dr. Jordan's assistants, Elmer Farmer, expert electrician, has invented an electrical machine for branding seals, and if it prove satisfactory it will do a great deal toward settling the seal question. There is a possibility that the female seals may be coralled on one of the islands during the sealing season.

AN International Congress of Technical Education took place in London on June 15th, 16th, 17th and 18th. M. Saignat, the retiring President, made some remarks in introducing the President, the Duke of Devonshire, who addressed the Congress at length. Papers were presented by Professor Otto N. Witt, of the Berlin Polytechnic Institute; Dr. H. E. Arm-

strong, Sir Henry Roscoe, Professor G. Lunge of Zurich, Mr. G. R. Redgrave and others. The United States appears not to have been represented by delegates.

A DECREE has been signed by the President of the French Republic regarding medical experts before the Courts of Algeria. It appears that such experts must be appointed at the beginning of each year, and receive a fixed compensation for such services as are rendered.

UNIVERSITY AND EDUCATIONAL NEWS.

THE will of the late Dr. George Edwards leaves $50,000 to Princeton University for the endowment of a chair in American History and $2,500 for other purposes.

AT the commencement exercises at Beloit College, Dr. G. K. Pearsons presented the College with $30,000 to build a woman's hall. Dr. Pearson's previous gifts to the College amount to $250,000.

PRESIDENT SEELEY announced at the commencement exercises of Smith College that $10,000 had been offered to the College for the erection of a building for the scientific laboratories on condition that $9,000 should be subscribed by the alumnæ.

UPON the recommendation of the Faculty of the College of Physicians and Surgeons, Columbia University, the title of the chair of chemistry and medical jurisprudence was changed to that of physiological chemistry. The chair has not yet been filled.

DR. THOMAS S. FISKE, adjunct professor in mathematics in Columbia University, has been promoted to a full professorship.

PROFESSOR M. V. O'SHEA, of the Buffalo School of Pedagogy, has been appointed to the chair of pedagogy in the University of Wisconsin.

THE trustees of Oberlin College have elected as President, Dr. William Slocum, now President of Colorado College.

DR. FUCHS has been promoted to an associate professorship of paleontology in the University at Munich. Dr. G. Boccardi has been appointed associate professor of microscopical anatomy at the University of Naples, and Dr. J. Szadowski associate professor of geology at the University at Klausenburg. Dr. E. Wiechert has qualified as docent in physics at the University at Göttingen, Dr. Adolf Marcuse as docent in astronomy in the University of Berlin, and Dr. Max Dettrich as docent in chemistry in the University at Heidelberg.

DISCUSSION AND CORRESPONDENCE.

PALEONTOLOGICAL AND EMBRYOLOGICAL METHODS.—A REJOINDER.

ALTHOUGH I would have preferred to postpone the discussions on the systematic position of Tarsius which was commenced a few months ago (SCIENCE, February 12th, April 2d, April 23d) till the publication of a more extensive memoir which is at present in preparation, I may yet be allowed to say a few words in reply to Mr. Earle's "claim that the paleontological method in determining phylogeny is more nearly accurate than the embryological, as in the latter many characters are lost and innumerable cœnogenetic variations are introduced which the embryologists often cannot distinguish from real homogenetic structures."

Leaving aside the somewhat loose constructions as to a 'method' in which 'characters are lost and 'variations introduced,' and considering it to be the writer's intention to point out to us that the facts which are brought to light by embryological research are for many reasons less reliable than those which we owe to paleontological science, I would wish the readers of SCIENCE to follow up that line of reasoning a little more closely. For it would seem to me that if we run a great risk of going astray in comparing the characters of the early embryos with each other because these characters are influenced by 'cœnogenetic variations,' it is *a fortiori* none the less dangerous to compare the adult structures which develop out of these embryonic rudiments and upon which the same caution should necessarily be brought to bear. In that respect a paleontologist can hardly be said to be in a safer corner than an embryologist.

When, however, the embryologist wishes to construct phylogenetic trees out of his ontogenetical data I quite agree with Mr. Earle

that he is on less stable ground than the paleontologist who, for example, in the case of the horse has dug up the actual tree out of the successive Tertiary strata. Ontogenetic development undoubtedly makes us acquainted with cænogenetic variations which have been the cause of many a premature conclusion. In Tarsius, however, it was not to any changes, hypothetical or otherwise, that attention was directed by me, but on the contrary to a *persistence* of a most unexpected *agreement* which reveals itself in the numerous details of a complicated arrangement, such as is only found in man, the monkeys (as far as investigated) and Tarsius. And whoever calls attention, as Mr. Earle does, to the liability of embryological structures to vary must necessarily recognize the high value of those cases in which there is evidence not of variation, but of stability. It is because of this stability that Tarsius should be placed somewhere between (though not, of course, in the direct line) the Anthropoids and their unknown Mesozoic ancestors with a non-diffuse placentation.

Mr. Earle's assumption that the diffuse stage of placentation of some of the Anthropoids is apparently directly comparable to that of the Lemurs is invalidated by recent researches on early human plastocysts, researches which all tend to confirm the objections against such comparison which were raised by me in 1889 (*Quar. Journ. of Micr. Science*, Vol. 30, p. 364 and p. 382).

Finally, Mr. Earle's contention "that it has not been shown as yet that the placenta in the lemurs is not a derivative of the chorion, as in the apes," although perfectly justified when it was written, is nevertheless unsupported by the actual facts.

Since Milne-Edwards has emitted the opinion which Mr. Earle cites in a footnote, the Lemurine placentation has been again described in the *Quart. Journ. of Micr. Science*, Vol. 36, p. 90, pl. 9–12. In addition to that I may now state, on the strength of observations as yet unpublished, that in a true Lemur, such as Nycticebus, the fusion of the vascular walls of the allantois with the non-vascular diplotrophoblast can be followed step by step. It is a totally different process from that by which the vascular chorion of man, monkey and Tarsius comes into existence.

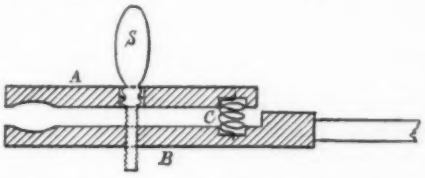
Another important point of difference between Lemurs and Tarsius which I have only lately been able to establish, thanks to the preparations just alluded to, is the presence in Nycticebus of a typical proamnion, so common among the lower Placental Mammalia and wholly absent in Tarsius and in man.

A. A. W. HUBRECHT.

UTRECHT, May 18, 1897.

A CLAMP FOR FRAGILE GLASSWARE.

WE have for some time been using a simple modification of the well-known wood clamp, which has given sufficient satisfaction to warrant a brief description. The clamp is constructed for holding light tubular glassware like Crookes' tubes, and it acts at once as a screw clamp and a spring clamp. The jaws, *A*, *B*, are set at any desirable distance apart by the screw, *S*. Loose pins allow *A* to rock slightly in the plane of the screw. The spring, *C*, in the rear presses strongly outward. When set, the jaws may thus be further opened (like a spring clip) by compressing the spring, *C*, with the hand. The jaws may be either flat or notched and need no packing.



This clamp has the following advantages: By aid of the screw the clamp may be adjusted to fit any tube up to over 2 inches for the ordinary size of clamp. The tube is removed or again inserted by mere pressure of the hand in the rear end of the jaws, *A*, *B*; the gradual counter-pressure of the spring prevents crushing of fragile apparatus, even when no cork or rubber cushion is used, while it can be intensified to hold very heavy objects.*

CARL BARUS.

BROWN UNIVERSITY, PROVIDENCE.

* The clamp can be obtained from Eimer & Amend, in New York.

MERRILL'S 'TREATISE ON ROCKS.'

To the Editor of Science: In animadverting on the defective English of Professor Merrill's 'Treatise on Rocks,' etc., in your current number, Mr. Woodworth says: "In the case of other quotations it is sometimes doubtful as to which author the work in a certain district is to be referred" (page 996). Will the author of this sentence kindly express his idea in German or French or Chinook, and thereby oblige half a dozen readers who find themselves unable to grasp his exact meaning?

W J McGee.

Washington, D. C., June 26, 1897.

THE TRIAL OF THE CROSSLEY REFLECTOR OF THE LICK OBSERVATORY.

A matter relating to work at the Lick Observatory (with reference to the trial of the three-foot reflecting telescope presented to us by Mr. Crossley, of England, in 1895) has lately been discussed in newspaper press-dispatches in rather a one-sided fashion. I desire to say that the questions involved have been passed upon by the Regents of the University of California, who have, unanimously, approved my action. The decisions of the Regents have not been spoken of in press-dispatches, which is my reason for writing this note.

Edward S. Holden.

The Lick Observatory, June 21, 1897.

SCIENTIFIC LITERATURE.

Our Native Birds of Song and Beauty. By Henry Nehrling. Published by George Brumder, Milwaukee. Vol. II., 4°. Pp. 453; 18 colored plates. Complete work in 16 parts, $16; bound in 2 vols., library binding, $18; handsome dark leather binding, $22.

The second complete volume of this admirable work, some of the separate parts of which have been already noticed in these pages, has now been issued to subscribers. The two volumes make a handsome addition to any library and should be in the possession of all lovers of birds. The work contains no technical matter, and little effort has been made to incorporate the latest facts bearing on the geographic distribution of the various species, the aim being to supply trustworthy accounts of the life histories of the birds in relation to their environment and in their relations to man. Mr. Nehrling has the instincts and sympathies of a naturalist. He is evidently a botanist and musician as well as an ornithologist, and his descriptions of bird life are generally woven in with pictures of shady ravines and forest glades or flowery dells where the birds and plants and landscape are seen together as they are in nature. His residence in widely separated localities—Wisconsin, Missouri, Texas and Florida—has enabled him to become familiar with a large proportion of the birds he treats, and his personal knowlege is supplemented by copious extracts from the writings of others. Special emphasis is given to economic relations and breeding habits.

As stated in notices of the earlier parts, the illustrations, all of which are colored, show the birds in natural surroundings and are of two kinds: plates of single species, mostly by Robert Ridgway; and mixed plates, by Mützel and Göring. Some of Mr. Ridgway's plates of single species are among the most charming bird pictures I have ever seen. Those of special excellence in the present volume are the Black Rosy Finch (*Leucosticte atrata*) and Dickcissel (*Spiza americana*)—the former on a mountain top, the latter in a clover field. Three of the mixed plates by Mützel are worthy of special mention; Plate XXI, a group of showy winter birds—the Pine Bullfinch, White-winged Crossbill, Redpoll, Evening Grosbeak, Nuthatch and Chickadee—in the top of a snow-covered spruce; Plate XXVIII, the Blue Grosbeak and three species of *Passerina*—the Indigo Bird, Painted Bunting and Lazuli Finch; Plate XXXVI, six species of brilliantly colored Woodpeckers.

Even in the mixed plates the attempt has been made to group the species in appropriate surroundings, and much pains has been taken with the landscape and vegetation. Thus, Göring's picture of the Scissor-tailed Flycatcher, Green Jay and Verdin shows these birds (and one or two others) among cactuses, flowering agaves, and aborescent yuccas with a barren range of desert mountains in the background.

Although the plates are of uneven merit, even the poorest are sufficiently good to serve the purpose of identification and will be most

helpful to the student. We take pleasure in commending the work to nature lovers, and particularly to the large and healthful class of of out-door students of birds.

C. H. M.

The Present Evolution of Man. By. G. ARCHDALL REID. London, Chapman and Hall. 1896. pp. 370.

The work which has appeared with the above title consists in fact of two parts; up to p. 196 it has reference to organic evolution in general, and only the remaining portion, pp. 197–370, treats especially of the evolution of man. In the earlier part of the work there is given a very excellent discussion of the broad principles of evolution, and particularly of the reasons which lead to the conclusion that acquired characters are not inherited by other than the lowest organisms. The argument against the transmission of acquired characters, as ordinarily understood, appears to the writer conclusive, and he would commend it to the neo-Lamarckians for dissection. It is also well shown that under ordinary circumstances natural selection works upon normal variations, and not upon those which occur only at infrequent intervals. Much stress is also laid, very justly, upon the importance of characters which are normally acquired, and of the power of acquiring them.

An interesting argument runs as follows: Inasmuch as progressive or new variations may be in all directions, but atavistic variations are in one direction—towards the ancestry—there will be a tendency, in the long run, *in the absence of selection*, to revert to a more primitive condition, owing to the dominance of the atavistic variations. When the evolution has been very slow, as with certain Brachiopoda, the reversion will be scarcely noticeable, but when it has been very rapid, as with many domesticated animals, the reversion will be rapid and striking. All this appears to accord with the known facts, but to the present writer it seems an inadequate statement of the actual course of events. Mr. Reid says: "In every species natural selection as a cause of evolution, and atavism as a cause of retrogression, are constantly at war." It does not seem to me that this is necessarily the case, but that, on the other hand, atavistic variations may be themselves selected. The germ, it must be supposed, contains units representing many phases of existence, some of which have been held over, undeveloped, through many generations, while others are new. When one of the latter develops we say the variation is a progressive one; when the former develop we call the result atavism. It is reasonable to suppose that environmental and germinal selection are the factors which determine which of its possible developments the germ shall undergo. That is to say, there are two factors involved, one the relative vitality or growth-force of the several germinal elements, the other the environment favoring one or the other in their struggle This same struggle, in various phases, goes on through life; for example, many people have two or more talents, which cannot be fully exercised simultaneously; other things being equal, the strongest will prevail, but how often the environment steps in and dictates which of the possible paths of life shall be followed.

This being made clear, it is evident that atavism increases the range of possibilities of any given germ, and thus may be highly advantageous. Especially is this the case when the environment is changeable, as with seasonally dimorphic butterflies, one phase of which is probably in most cases older than the other. My own studies of bees have led me to believe that many of the specific characters had their origin in atavistic variations, because it often happens that a character will appear in two different groups independently, and yet be so striking and definite as to suggest that it must have existed in a common ancestor, though not in the immediate ones.

How, then, as to atavism in the absence of selection? It is perfectly obvious that any given adult individual does not, under existing circumstances, represent the average potential, if one may so express it, of its race. In the first place, the individual is probably a survivor out of many—has been the subject of natural selection. In the second, it represents only one (selected) phase of the many that were possible to the germ. In the total absence of selection (an impossible thing) we should obviously

get the actual mean, or a series of individuals symmetrically arranged around that mean. This would involve more or less atavism, because the potentialties of the germs are derived from past generations, and include many repetitions, more or less complete, of phases which have lately, owing to the incidence of selection, been permitted no development. But I cannot see, with Mr. Reid, that there would be unlimited atavism, because when the atavistic changes had proceeded from B to A, the B-features would become ancestral, and a new atavism, from A to B, would appear. Thus at some point there would be reached a condition of equilibrium, and the tendency to vary in any particular direction would be lost. Existing species may be compared to elastic bodies under stress, the stress being natural selection. When the stress ceases, there is contraction until the condition of equilibrium is reached, but certainly not indefinite contraction.

The second portion of the book consists of a discussion of human evolution against zymotic diseases and narcotics. Mr. Reid argues that the way to tell in what direction the evolution of a species is tending is to observe the causes of mortality. Applying this test to man, he concludes that as disease is the principal cause of death it must be against disease that we are evolving. He then proceeds to show that those races which have long been subjected to a particular disease, *e. g.*, malaria or consumption, have acquired a relative immunity from it, or at least a power of enduring its attacks. The whole matter is set forth very clearly and is well worth reading. While it cannot be denied that the factor of zymotic disease is an extremely important one, hitherto generally overlooked in discussions on human evolution, I must say that I think Mr. Reid himself has overlooked some not less important factors. The 'social efficiency' of a people, as Mr. B. Kidd rightly insisted, is a factor of immense importance. Thus, in this very matter of zymotic diseases, how greatly is the death rate in an epidemic (and the existence of the epidemic itself) influenced by the social condition of the people, and even by their intellectual and moral condition, as reflected in the municipal government and sanitary arrangements! Thus, while, as Mr. Reid shows, the negro is under identical conditions far less affected by malaria than white people, Dr. J. S. Billings has to report, in summing up the statistics for 28 cities of the United States, that the deaths from malarial fever were more than three times as numerons, in proportion to the population, among the colored people as among the whites. It is fair to state that under the term 'colored' he includes the Chinese, etc., but the difference is conspicuous in those cities which are known to contain a large negro population. Mr. Reid may reply that sanitary arrangements and forms of government, affecting all, select nobody; but certainly they do select the citizens of one town, State or country as against others, or those of the rural districts as against the towns. Thus the population of large cities, like London, is perpetually replenished from the rural districts. A pure-bred cockney of the fourth generation is said by J. M. Fothergill to be very rare. Further, as Dr. Billings has shown (11th Census of the United States) the death-rate in every large city varies enormously in the different wards, and this must be due mainly to the mode of life of the people, their food, sanitation, and other matters directly connected with social efficiency. Still again, there is a tremendous proportion of infant mortality, a large part of which must be due to unfavorable conditions of the mother or child. Thus it must necessarily be that, while there are inequalities in morality and intelligence, the most moral and the most intelligent races or groups of people will be favorably selected in the struggle for existence.

The whole subject is one of immense complexity, and in studying the statistics one has to be constantly on the lookout for sources of error, which are numerous and confusing. But there is no doubt that Mr. Reid's discussion is a valuable one, if only it draws attention to matters which have been too much overlooked.

The final portion of the book, treating of evolution, against alcohol and other narcotics, seems to me to contain a fundamental error. It is assumed that a desire for alcohol is inherent in the human race, and that, since the substance cannot be banished, our only salvation is to gradually acquire a toleration of it, as of a

zymotic disease. It is shown that certain races are on the way to acquire such toleration, and in the presence of alcohol suffer much less than others. An obvious difficulty here is to explain how, under the influence of natural selection, this highly pernicious craving for alcohol arose. Mr. Reid says: "It can have arisen only as a bye-product of mental evolution, a bye-product which, in the absence of narcotics, was harmless, but which in the presence of them is harmful." This surely is a very far-fetched supposition, since it is implied that the craving for alcohol developed in the absence of that substance! To the present writer it appears evident that the human race has no natural craving for alcohol at all, but it has a craving for excitement and other states of mind which may be induced artificially, and that when the natural exercise of highly valuable faculties is denied, as is so often the case in our present civilization, artificial means, often highly injurious, will be resorted to. It will apparently sometimes happen that when an artificial stimulus is persisted in for a long while, and is not in itself injurious, it will become a necessity, just as clothes have become necessary to a large portion of mankind. As an instance of this, we may cite the use of hot flavoring substances (as pepper) by inhabitants of warm countries to promote digestion. It is conceivable also that a race might acquire considerable toleration of alcohol, and at the same time lose the power of acting efficiently except under its influence, so that a member of the race, separated from his bottle, would be powerless! But it is perhaps more likely that the process would be just that which Mr. Reid describes as inevitable, namely, that the desire for alcoholic stimulation would be lost, while at the same time the effects of drinking a given quantity of alcohol would become less. This is regarded by Mr. Reid as very desirable, but what if it means the destruction of a valuable faculty, which, rightly exercised, might have been once more, as originally, of great importance to the race? Put it in this way: The wretched laborer of a crowded city, overworked and underfed, desires to escape from his environment—desires, if only for a brief period, to be free. Alcohol gives him a temporary means of escape, but at a frightful cost. It is deplorable that he should seek it, but how much more deplorable it would be if he should cease to care—if he should become degraded to a mere machine, accepting without thought the suppression of three-fourths of his natural activities? Surely the remedy is not, as Mr. Reid supposes, to eliminate those who wish to drink, but to find the means of living full and active lives, in the natural exercise of all our functions.

T. D. A. COCKERELL.

MESILLA, N. M., June 8, 1897.

SOCIETIES AND ACADEMIES.

MEETING OF THE NEW YORK SECTION OF THE AMERICAN CHEMICAL SOCIETY.

THE New York Section of the American Chemical Society held its June meeting on the 11th inst., preceded by a dinner, at which thirty members participated, including the President of the General Society, Dr. C. B. Dudley. The meeting was called to order in the chemical lecture room of the College of the City of New York, at 8:30, by the chairman, Dr. William McMurtrie, who then invited Dr. Dudley to preside.

The death of Professor C. R. Fresenius at Wiesbaden was announced, and the Secretary was authorized to cable the regrets and condolence of the Society to his sons. An obituary notice of Dr. Carl H. Schultz was read by Dr. A. P. Hallock, after which the papers of the evening were read as follows:

F. S. Hyde, 'Comparative Tests for Identification of Some Medicinal Carbon Compounds.' E. G. Love, 'Note on Analysis of Cheese.' Benj. C. Greenbergh, 'Determination of Dextrine in Presence of Sugars.' Gustav Volckening, 'Novel Mechanical Arrangement of Fat Extraction Apparatus.' L. Reuter, 'Demonstration of Some Chloroform Compounds and of Some of Baumann's Thioaldehydes.' W. E. Chamberlin, 'Calibration of Volumetric Apparatus.' C. A. Doremus, 'Method of Collecting and Analyzing Gases contained in Canned Goods.'

It was stated by the chair that forty papers had been presented before the Section during the winter, a number considerably in excess of

any previous session, and the attendance at the meetings had averaged about fifty, also an increase over previous records. The Section was then adjourned until October.

DURAND WOODMAN,
Secretary.

TORREY BOTANICAL CLUB, MAY 11.

DR. N. L. BRITTON presided in absence of the President. Three new members were elected. Three successful field-meetings were reported. Resolutions were adopted commemorative of Dr. Emily L. Gregory, the late honored professor of botany at Barnard College, an active worker in the Club. Announcement was made of the recent gift, by President Low to the botanical department of Columbia, of a valuable set of water-color plates prepared by the late lamented Wm. Hamilton Gibson, in illustration of his projected work on mushrooms.

The paper of the evening followed, by Mr. Marshall A. Howe, entitled 'A Preliminary Comparison of the Hepatic Flora of California with that of Europe and of the eastern United States."

The total number of Californian species now known is 77, of which 45 occur in the Mediterranean region, but only 37 in the Gray-Manual region of the United States.

It was shown that the hepatic flora of California has more in common with that of northern and central Europe than with that of the eastern United States, and is still more allied to that of the Mediterranean region. In particular, species of *Asterella* and *Riccia* are better developed in California and southern Europe than in the eastern United States.

The apparent absence in California of *Bazzania* and *Mylia*, which are especially characteristic of medial and boreal regions, serves to heighten the similarity to southern Europe.

The paper was followed by exhibit of photomicrographs of sections of Cryptomitrium, illustrating the development of the archegonia. In the discussion following, Professor Underwood said that the Hepatic species are most numerous in the Amazon region, the eastern slope of the Andes, and in Java. Insular tropical regions have furnished many where examined, as Cuba and Jamaica. Quite a number are peculiar to Australia. New Zealand is well supplied with species. Many have been recently collected in Africa, and have been described by Herr Stephani, of Leipsic, whose industry has doubled the number of described Hepaticæ. As a whole, the maximum development of the Hepaticæ is tropical, though some genera and certain groups within genera are wholly high-temperate or subarctic.

Professor Britton, remarking the indications of circumboreal and circumtropical distribution of certain species, referred to the argument for an equatorial distribution of flowering plants and of ferns, and queried if there were anything corresponding among Hepaticæ. He expressed the belief that it is the immediate environment which at present exerts the principal influence on distribution, whatever the original cause or mode of distribution may have been.

Professor Underwood referred to the influence of the Gulf Stream in permitting the existence of the subtropical genus *Lejeunia* on the coast of Ireland, a genus not elsewhere found in Europe. Comparing the Hepaticæ of Florida, they are only in part known; a few species are in common with the Appalachian flora; most of the Florida Hepaticæ are close-creeping forms found on bark, as *Frullania* and *Lejeunia*, having water-sacs on their leaves as aids in resisting drought. Some tropical *Marchantiaceæ* occur in Florida, and also, especially, species of *Riccia* and *Anthoceros*. *Thallocarpus* is known only from Florida and South Carolina.

EDWARD S. BURGESS,
Secretary.

NEW BOOKS.

Formation de la Nation Française. GABRIEL DE MORTILLET. Paris, Alcan. 1897. Pp. iv + 336. 6 Fr.

Introduction to the Study of Economics. CHARLES JESSE BULLOCK. New York, Chicago and Philadelphia, Silver, Burdett & Co. 1897. Pp. 511. $1.28.

Grundprobleme der Naturwissenschaft. ADOLF WAGNER. Berlin, Gebrüder Borntraeger. 1897. Pp. vi + 255.